Report:
Assessing the Impact of the April 2005 Amendments
To the NLRB Representation Case Procedures

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I was asked by the AFL-CIO to analyze data concerning National Labor Relations Board representation proceedings in relation to the AFL-CIO's submission of information to the Board pursuant to the Board's request for such information published in the Federal Register on December 14, 2017. 82 Fed. Reg. 58783. This report is the product of that analysis.

QUALIFICATIONS

My qualifications are set forth in my CV, which is attached hereto as Exhibit 1.

DATA ANALYZED

In order to produce this report I was given access to data provided to the AFL-CIO by the NLRB pursuant to a Freedom of Information Act request. The AFL-CIO's request is attached hereto as Exhibit 2. The NLRB's response summarizing the data that was produced is attached hereto as Exhibit 3. In most of the analyses described herein, I consider closed cases from the NLRB's NxGen case-tracking database, covering the five-year period from October 2012 to October 2017. This gives 2.5 years of data on either side of the rule change to compare.

SUMMARY OF ANALYSIS

My analysis focused primarily on determining what changes were associated with the Board's adoption of amendments to its representation case procedures that took effect on April 14, 2015. I analyzed the data to determine if the amendments were associated with any changes in the time needed to process representation cases from petition filing to election, the outcome of elections, the number of petitions filed, the number of employees gaining representation through the election process, the percentage of cases requiring the issuance of a decision and direction of election, the overall time from petition to case closing, and several other variables.

In general, I concluded that the amendments are associated with a significant decrease in the time between petition and election and the time between petition and the closing of cases, but that the amendments are not associated with any other significant changes in case processing variables or outcomes.

In addition, I analyzed whether the Board's decision in *Specialty Healthcare & Rehabilitation Center of Mobile*, 357 NLRB 934 (2011), issued on August 26, 2011, was associated with any changes in the size of units in representation proceedings.

METHODOLOGY

I.   What it means to have an effect: Election Lag vs. Petitions Filed

The subject of this report is changes and effects associated with the April 2015 amendments of the representation case procedures. In drawing comparisons, I want to be clear about what it might mean for the rule change to "have an effect" on something. Evidence for an effect should satisfy several criteria. First, the effect size should be substantively meaningful, not just statistically significant. Second, the effect should be clearly observable in simple descriptive statistics. Third, the effect should be concentrated temporally around the rule change. These criteria are important for distinguishing between effects that are probably *because* of the rule change, and effects that happen *during* or *alongside* the rule change.

I describe two empirical examples at the start in order to illustrate this distinction. The advantage of doing so is to give the reader a sense of how I evaluate whether a time trend observed in the data can or should be attributed to the rule change. By going into detail on these first two examples, I can compare what real effects and statistical artifacts look like. When I present other analyses, I can refer back to the types of analyses done on these first two.

I.A. Time between filing and election

I define the term "election lag" as the time between when a petition is filed and when the NLRB-supervised election is conducted. I operationalize election lag as the number of days between the petition filing date and the date of the election. (Obviously, this is only defined on cases that resulted in elections.) There are 8,335 cases in the analysis period with information on filing and election dates. Election lags have a skewed distribution: most elections are held within a few weeks or at most a couple months of the petition filing, but a small number take place much longer after petition filing. Consequently, the average election lag tends to be much greater than the median lag. Thus, in Figure 1 I present monthly observations over five years for the median election lag, as well as the interquartile range. The red line in Figure 1 and subsequent figures indicates April 2015, when the rule change was implemented.[1]

The advantage of presenting the data this way is that it becomes immediately obvious that there was a major reduction (nearly half) in election lag in the immediate wake of the rule change. The reduction happened quickly and persisted for the rest of the study period. Another thing Figure 1 shows is that there was no underlying time trend in election lag, net of the rule change itself. Median lag hovered under 40 days for 2.5 years before April 14, 2015; it hovered over 20 days for 2.5 years afterward.[2] This makes it more straightforward to associate the

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[1] Because Figure 1 and subsequent figures collapse data to a monthly observation, the point intersecting that red line represents the average from 1 to 30 April 2015, and other months are calculated accordingly. That said, adjusting all the data to consider 30-day increments before or after the rule change produces substantively identical results to those seen here.

[2] The spike in delay in late 2013 appears to be the product of a single dispute between two unions concerning the right to represent certain healthcare workers in California. Removing those cases from the analysis does not change the pattern of results.

reduction in election lag with the rule's implementation. This is nearly a textbook example of what an "effect" should look like.

I.B. Number of petitions filed

Compare the time to election with the number of petitions filed every month with the NLRB. Both are simple variables. Contrary to election lag, there is no evidence that the rule change affected the number of petitions filed. However, if you do your analysis wrong, you can produce what looks like a significant effect. I think it is useful to go through one such analysis in detail to see what the pitfalls are and to give the reader a sense of what is happening behind the curtain.

Figure 2 shows monthly counts of petitions filed. I again flag April 14, 2015, when the rule was implemented. Looking at these data, no long-term pattern jumps out. The first and most straightforward statistical test of whether the rule change had any effect is a *t*-test of differences in means. We can look at the average number of petitions filed per month after the rule change, compared to before the rule change, and test whether the difference is larger than we would expect by chance. It is marginal. On average, 212 petitions were filed per month before the rule change, 197 per month afterward. The standard errors around these numbers are 5.5 and 5.4 respectively.[3] There is a 5.03-percent probability that we could observe this difference in the averages just by chance.

This difference in the before-after data could also just reflect a longer-term trend in these data. We can regress petitions filed on time, to see if there is any simple, linear trend evident in this time series. That is, we can estimate the parameters of the model:

$$Petitions_t = \beta_0 + \beta_1 FilingMonth_t + \epsilon_t$$

There is apparently none. A linear regression of petition counts on filing month yields a coefficient of -.38 for filing month, with a standard error of .22. The probability that we could get that negative trend by chance, given the underlying variability in the data, is 9.5 percent. The shorthand for this is (β_1=-.38, p < .095). For traditional standards of statistical significance, we would like to see p < .05; we are not close here. (Full regression results are available on request.)

Something to notice in figure 2 is that there is marked seasonality in these data. Petitions are more likely to be filed early in the year and especially uncommon in December. It could be that the time trend here is *not* simply linear, that it is swamped by the seasonal swings in petition counts. One way to account for this is to allow each calendar month to have its own effect, and then look for a time trend. That is, we can subtract the mean for December from all of the December observations, the mean for January from all January observations and so on, and then see if there is a time trend in these "de-meaned" observations. This is equivalent to regressing counts of petitions on the filing month and indicator variables (equal to 1 or 0) for each month:

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[3] I round statistics here.

$$Petitions_t = \beta_0 + \beta_1 FilingMonth_t + \boldsymbol{\beta Month} + \epsilon_t$$

(The third term on the right-hand side is in bold to indicate that it is actually a vector of eleven variables. You have to omit one month to compare the others to; I chose January.) If you do this, there is much stronger evidence for a time trend: β_1=-.50, p < .003. It does seem that monthly petition counts have been falling over time, though the effect is small, about half a petition per month on average. The question then becomes, can we attribute this decline to the rule change?

We can now compare the periods before and after the rule change, taking these monthly idiosyncrasies into account. I do this by including an indicator variable for whether the observation was from before or after the change.

$$Petitions_t = \beta_0 + \beta_1 FilingMonth_t + \boldsymbol{\beta Month} + \beta_2 PostRule + \epsilon_t$$

If I do this, that indicator is not significant (β_2=-12.01, p < .30), and the apparent downward trend in petitions over time *also* becomes insignificant again (β_1=-.20, p < .53). Given that the post-rule indicator and the filing month are both time variables, it is not too surprising that neither is significant when included in the same model. They measure very similar things, and it is difficult to attribute variance to either one of them.

As a last and most "sophisticated" test, we might let the time trend *itself* vary before and after the rule change. That is, rather than just including an indicator for whether an observation is from before or after the change, which is equivalent to letting the intercept of the time trend change but keeping the slopes equal, we can multiplicatively interact the indicator with the time trend, allowing the slopes to vary:

$$Petitions_t = \beta_0 + \beta_1 FilingMonth_t + \boldsymbol{\beta Month} + \beta_2 PostRule + \beta_3 PostRule \times FilingMonth_t + \epsilon_t$$

This allows us to interpret the time trend before the rule change as β_1, the "bump" (if any) from the rule change as β_2, and the time trend after the rule change as $\beta_1+\beta_3$. In this model, there *is* a statistically significant increase in the immediate wake of the rule change, and the slopes *are* different. Whereas before the rule change β_1=.74, p < .07, afterward $\beta_1+\beta_3$=-1.25, p < .00.

I combine all of these results in figure 3. Panels A and B at the top of the figure do not support any impact from the rule change. In A (the simplest linear model), the range of uncertainty around the fitted estimate includes zero. Accounting for the calendar month (panel B) helps reveal a negative temporal trend, and greatly increases the model's overall explanatory power. Notice that the simple linear model in panel A only explains 4.7 percent of the total variation, while adding in the controls for month in panel B explains 49.4 percent of it. By contrast, adding the indicator for the rule change in panel C does little: the variance explained only rises to 49.5 percent.[4] Allowing the time trend to differ on either side of the change seems

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[4] Adding an indicator of the rule change to a model of *election lag* that already controls for the time trend and calendar month raises the variance explained from 71 to 91 percent.

more important: the "bump" in April 2015 is marginally significant, the time trends themselves differ, and the variance explained rises to 58.9 percent.

Should we conclude from this that the rule change led to an initial increase in filing rates but then encouraged declines? No. Models like these are sensitive to outliers, and it is possible that the lower counts in 2016 and 2017 entirely account for the estimated difference. Whenever one does a comparison like this, it is important to also do a *placebo* test. Do we find similar results if we treat a *different* month as the break point?

With this in mind, consider Figure 4. This figure reports the estimated coefficients from 37 different models. Each one models the count of petitions filed as a function of the time trend, calendar months, the rule change, and the interaction of the change with the time trend, as in panel D of Figure 3. The difference is that I vary the month of the rule change, ranging from eighteen months before the actual change to eighteen months after. Figure 4 reports the resulting coefficients and standard errors. The placebo test is really important here, because it demonstrates that we can find negative "effects" from placebo rule changes as well, ranging anywhere from twelve months before to six months after the actual change.

What I have done in this section, statistically, is build a mirage. When you have a lot of variables and a lot of choices for the model you might estimate, you can often find statistically significant effects where none exist. Partly this is a function of hypothesis testing itself. If you count findings as significant when the probability of their appearing by chance is 5 percent or less, then up to 5 percent of the time you will "find" a significant result where none actually exists. Other times you can find a correlation that confirms what you were expecting, and fail to see whether other explanations could produce the same result. The "effect" of the rule change on petitions filed presented in panel D of Figure 3 is statistically significant, but it is not substantively large (half a petition per month); it is not visible in simple descriptive analyses (we needed a fixed-effects regression with an interaction term to find it); and it is not temporally concentrated around the theorized cause (break points up to a year before the rule change are also statistically significant).

This is why I always prefer to visualize the underlying data. A large and important effect is likely to jump out in simple analyses, like the plot of the variable over time. This holds when we look at the time between petition filing and elections, as in Figure 1. By contrast, just looking at the data in Figure 2 should make us suspicious of any estimated effects on the number of petitions filed. It just doesn't pass the reasonableness check.

This section is long, given that my conclusion is that the rule change had no effect on the average monthly number of petitions filed. I think it is important though to explain what I mean when I say there is no effect, and to explain how I test this.

In the rest of this report, I do not go into nearly so much detail for each analysis. Instead I present aggregate results and my opinion of whether there is any evidence of an effect from the rule change. For each variable, I have done the sorts of analyses that I show here in order to form my opinions.

II.   Further analyses of the rule change

II.A. Election outcomes

We saw that elections happen in a shorter interval after the rule change. Do the outcomes differ? To study outcomes, I focus on the 8,335 cases in the study interval that have an election date recorded. I count as union won any such case whose closing reason is "Certification of representative"; any other closing reason I count as a union loss. (The vast majority of the remainder are "Certification of result.") I plot the results in Figure 5.

Eyeballing the raw data in Figure 5, the trend looks non-linear, rising before the rule change and possibly declining afterward. Rather than play around with linear regressions (which require a constant trend over time), I also fit a LOWESS, or locally weighted regression line, to the data.[5] That line agrees with the initial impression: union win rates rise through 2013 and 2014, roughly until the rule change, and decline afterward. Union win rates in elections have been climbing for a generation,[6] but the rise in the 2.5 years before the rule change seem to have been reversed since.

As always, though, we should ask whether a difference seen *after* the rule change is probably a *result* of the rule change. In this case, it is relevant to consider the starkly low average union win rates in October and November 2016. Cases where the petitions were filed in these months were distinctly less likely to result in union victory. These outlier months may pull the entire estimated trend downward. Accordingly, I also fit a LOWESS curve that excludes October and November 2016; this is the dashed line in Figure 5. That latter curve's trend is statistically indistinguishable from zero after the rule's implementation.

In my opinion, there is no evidence that union win rates declined after the rule change, setting aside the months of October and November 2016.

II.B. Pro-Union vote share

For the average pro-union vote share, I turn to the tally file, which contains records published between late 2010 and early 2017. There are 10,285 unique case numbers in this file, but 11,192 records because some cases have multiple units and elections may have multiple tallies. (Multiple tallies can be recorded for example when there is a recount. More than 90 percent of cases, though, have a single unit and a single tally.) For this analysis, I have kept the last recorded tally for each unit. This yields 10,461 elections. I also focus on elections where there is only one union on the ballot. There are 500 units with more than one union on the ballot, leaving 9,961. Of these, 8,191 were published in the five-year window around the elections rule change.[7]

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[5] Locally weighted regression calculates the slope of the regression line around each observation as a moving average of nearby observations, giving less weight to more distant observations. This allows its slope to vary over the observed data, and is a good exploratory approach to follow before assuming some more restrictive (if computationally simpler) parametric form like linear or quadratic effects.

[6] I have previously studied union win rates in representation elections going back to the early 1960s. Average win rates declined from over 60 percent in the early 1960s to about 45 percent in 1982. They then began climbing, reaching 55 percent by 1999 (Ferguson, 2009: 66). I extended that analysis using data from the NLRB's CATS and NxGen case-tracking systems, and found that average win rates had climbed above 70 percent by the end of 2016 (Ferguson, 2018: 4).

[7] There are 406 cases where the vote share, calculated as the votes for Labor Organization 1 divided by the number of valid votes cast, is greater than one. I set these aside when calculating trends, though their exclusion has almost

There are two main ways to calculate vote share: as an average of election results, or as a size-weighted average of election results. The latter is useful for a sense of the share of *all voters* who supported unions, but can mis-estimate the number of cases that unions actually won or lost. I prefer to take the unweighted average of vote shares across establishments, and then separately analyze the effect of size on vote share.

Figure 6 plots average vote shares for the resulting 7,785 cases. Unions' vote shares in the cases that went to election rose, on average, over the entire interval considered. By late 2017, unions won about 63 percent of the vote. This effect over time is substantively important and statistically significant, but it is uncorrelated with the rule change. The estimated slopes of the trend before and after the rule change cannot be distinguished from one another.

II.C. Employees represented

Having looked at the win rate and the vote share, I then calculate the count of workers who gained representation each month. This is straightforward, since it is just the number of eligible votes in elections counted as union won when generating Figure 5.

While the union win rate has risen over time, the number of workers gaining representation per month has not. As Figure 7 shows, the estimated linear effect of time on workers organized is indistinguishable from zero. Given that Figure 5 showed an increasing win rate, this implies that fewer elections are being held, that the unit size in elections is trending downward, or some combination of the two.

There is a spike in the count of workers gaining representation, though, in the month immediately after the implementation of the rule amendments. Its inclusion does not bias the estimate of the time trend plotted here—that is, setting aside May 2015 when regressing workers gaining representation on time still produces a zero, rather than a negative, trend. But the spike is still worth pointing out. This increase in workers gaining representation seems to be due not to a sudden increase in unit size in the elections that month, but to a sudden increase in the number of elections held that month. While there is no significant trend in the number of elections held per month over time (There were 137 per month on average), there is a significant spike in May 2015, and the average lag between petition filing and election in that month is significantly larger than before or after. Other than this one-time effect, though, there seems to be no impact on the number of workers gaining representation after the elections rule change.

If there is not a significant trend in the number of elections but an increasing union win rate, the fact that there is no significant increase in the number of workers gaining representation monthly would seem to imply that unit size has been decreasing. Figure 8 presents the median unit size over time, with the interquartile range. There is a significant negative trend in those data, but it is not substantively important and not related to the rule change. Median size is declining by just .05 workers per month, or by three workers over the entire five-year period. Notice though that, even in the interquartile range, is considerably more variability on the larger than the smaller side of the median. This strongly suggests that

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no impact on the bigger picture. While the average pro-union vote share over the study interval is 62.1 percent if they are included, it is 61.7 percent when they are excluded.

the negative trend in the *average* unit size (which is larger) is driven by a handful of months where larger elections happened, and those months' being clustered before the rule change.

I was also asked to determine if any changes in the mean or median unit size is associated with the Board's decision in *Specialty Healthcare & Rehabilitation Center of Mobile*, 357 NLRB 934 (2011), issued on August 26, 2011. As Figure 9 demonstrates, the decision is not associated with any discernable change in unit size.[8]

II.D. Cases proceeding to election; cases proceeding to withdrawal

Monthly withdrawal rates, measured as the percentage of cases with closing reason recorded as "Withdrawal Adjusted" or "Withdrawal Non-adjusted," also trended downward throughout the study period. Similarly and somewhat necessarily, the rate of petitions proceeding to election climbed throughout the period. See Figure 10. There is no evidence that withdrawal rates began declining faster, or election rates began rising faster, after the rule change than before.

II.E. Case Lag

The length of representation cases, measured as the number of days between initial petition filing and the closing of the case, was lower on average after the rule change: 77 days before, 56 days after (p < .00). I visualize case lag in Figure 11 using the median and interquartile range because, as with election lags, the mean here is quite skewed. The effect is substantial and concentrated around the rule implementation. There is no time trend in median case length net of the rule change.[9]

Obviously, we would expect such declines to be concentrated among cases that went to elections. To check this, I split the data into cases that resulted in elections and cases that did not. These are also plotted in Figure 11. Indeed, the decline is largest among cases with elections, but there is also a significant decline in case length among cases that do *not* result in elections. (Proportionately, the decline is comparable.) There is some evidence that the election rule amendments are not just shortening time to election but also wrapping up other cases more quickly.

II.F. Stipulated Elections versus Directed of Election

I operationalize a petition's resulting in a stipulated election agreement by the case having a "stip approved date" recorded. I operationalize directed elections by the case having a "DDE approved date" recorded.

Three quarters of the records have one of these notations recorded. Of the remainder, the vast majority closed by decision and order, withdrawal or dismissal. Of the cases where the

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[8] Figure 9 draws on the combined panel of representation cases I assembled from CATS and NxGen data for an earlier analysis. See Ferguson (2018).

[9] The *mean* case length has a negative time trend, which reflects a smaller share of outlier, long-running cases over time. However, this downward trend began before the rule change and is not correlated with it.

closing reason indicates that there was an election, only .75 percent are missing this information.

Figure 12 plots the monthly share of cases with stipulated, as opposed to directed elections. There is no evident time trend in these data, around the rule change or elsewhere.

II.G. Trends in pre-election hearings

Neither the probability of a case's having a pre-election hearing nor the average length of those hearings changed, over time or in the wake of the rule change, as shown in Figures 13 and 14. By contrast, the length of time between petition filings and the opening of a pre-election hearing, where one was held, *did* decline. Figure 15 plots the median lag between a case's petition filing and the opening of a pre-election hearing, with the associated interquartile range.

Figure 15 is a textbook example of why it can be important to pay attention to the median as well as the mean. Means are more sensitive to outliers, and at the upper range of the data there can be some incredibly long delays. Such delays exist before and after the rule change; yet the median delay was only 14 days before the rule change, and it shrank to 11 days after the change. That reduction is substantial, given the original delay length.

While the rate of petitions' resulting in pre-election hearings and the length of those hearings was virtually unchanged by the rule implementation, the proportion of times when briefs were filed after the hearing declined markedly. Figure 16 shows the percentage of cases with pre-election hearings in which post-hearing briefs were subsequently filed. Whereas the rate was near 50 percent on average and climbing before the rule change, it fell to about 20 percent afterward and has hovered around that number. This applies to briefs filed by all parties.

II.H. Pre-election requests for review

Over the study interval, up to 7.6 percent of the petitions filed in a monthly period involved a pre-election request for review; the monthly average was 2.6 percent. These requests were granted 30 percent of the time, on average. There is no evidence of any time trend in these figures.

II.I. Objections after elections

There are four fields in the data that record objections: by employers, petitioners, unions, and "others." I focus here on objection rates where the filing party is identified as one of the first three categories.

I plot the objection rates, by filing party, in Figure 17. It is important to note the different vertical scales in these plots. Objections by employers are by far the most common. Over the study interval, more than 85 percent of elections have employer objection filings recorded. Petitioners filed objections in a third of elections, while union-filed objections appear in just 5 percent.

Evidence of any impact from the elections rule changes here is mixed. In Figure 17 I plot trends from the best-fit regression models atop the raw data, as well as 95-percent confidence intervals and the "null" lines associated with a zero relationship. The employer objection rate rises throughout the interval (β_1=.0008, p < .004), but there is no evidence that the objection rate changed in response to the rule change (I.e., the interaction of the indicator variable for the rule change with the time trend is not significant). On the other hand, there is no significant time trend associated with petitioner objection rates (β_1 = -.0008, p < .23) but the indicator variable on the rule change is significant (β_2 = -.047, p < .05). There is no evidence of any time trend in union objection filings. In short, there is evidence that petitioners' objections are slightly less common (32 rather than 33 percent) after the rule change, but employer and union filings show no response.

II.J. Trends in post-election hearings

The probability of a case's having a post-election hearing declined over the study interval, as shown in Figure 18. That decline is uncorrelated with the implementation of the election rule changes, though. Despite some exceptionally long post-election hearings in late 2016, evidenced in Figure 19, the average number of days in a post-election hearing has not tended to rise or fall during the interval.

II.K. Blocking charges and blocked cases

I count a case as blocked when the "blocking case" field is non-empty. The resulting monthly counts are displayed in Figure 20. The count of blocked cases has declined significantly over time, but this does not appear correlated with the implementation of the election rule changes. If anything, there appears to be a significant break point in the average block-charge filing rate about a year *after* the rule was implemented: the counts of blocking charges and blocked cases both decrease significantly from the spring of 2016 onward, though there is no evidence of different time trends before and after that break.

III.   Trends specific to different parts of the rule change

Here I record some additional analyses of the data. Several of these are summary statistics for the pre- or post-rule-change period, where a time trend or differences in the time trend are less important. Thus, for these I quote numbers.

III.A. Position statements

The average position statement has an original due date of eight days after the petition's filing. Of the 2,598 position statements where there is clear information on the filing party (i.e., a submission date is recorded specifically for the employer, petitioner, or union), employers account for 1,954. The original due date for this statement is not always recorded, but it is available for 1,412 of the employer cases. In 352, or 25 percent, of these, the employer submitted the statement later than the original due date, by 18 days on average.

III.B. Delay between scheduled and actual hearing openings

Do pre-election hearings begin when scheduled? Since the rule change, there have been 541 pre-election hearings with scheduled dates. Of these, 529 also list their opening dates. In 507 cases, the hearing began when scheduled. In 16 cases, the hearing opened later than scheduled, while in 3 the hearing actually began earlier than the scheduled date. The gap in one of these cases is 730 days, which is either a massive outlier or simply a data error. Of the remaining 18 cases, the average difference is 13 days, with a range from -8 to 92.

III.D. Receipt of voter lists

The database has fields for the original and current due dates for voter lists as well as the actual date the list was received; however, those first two fields are often empty. While there is a date recorded for when voter lists were received in 4,781 cases, there are only original due dates recorded for 1,434. Whether this implies that, where the original deadline is not recorded, the list was received on time, it is impossible to say from the data alone. Of those 1,434, there is a discrepancy between the original deadline and the date received in 553 cases. However, in only 217 of these is the date received *later* than the original deadline. Thus, the average "lag" is only 5 days, with 95 percent of the lags falling between two weeks early and three weeks late—and 50 percent within *one* day on either side.

III.E. Determinative challenges in elections

Of 9,018 election tally records between October 2012 and October 2017,[10] 3,332 had at least one challenged ballot. Whether the challenge was determinative is recorded in 665 instances, of which 574 are yeses. This is 86 percent of the recorded instances, or 14 percent of all elections with challenges. Neither the rate of cases with challenged ballots, the rate at which determinations are recorded, nor the share of determinative challenges differs significantly on either side of the rule change.

IV.   Sub-analysis on RC cases versus all petitions

The above analyses have been calculated on all petitions filed in the five-year study window. It is worth checking whether the pattern of effects or non-effects is an artifact of combining different types of petitions, or whether the same pattern of results appears when just RC cases are considered. I therefore replicated all of the major analyses using just the RC cases, which comprise 81 percent of the sample.

Figures 1A through 20A correspond to figures 1 through 20 (for simplicity, I have not reproduced all 20 figures). Perusal of those figures will show that these patterns are largely driven by, and consistent with, patterns among RC cases. There are some level differences. For

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[10] There are of course more tallies than there are elections, because of reruns, runoffs and the like. For looking at challenges the tally, rather than the broader election, seems like the right unit to have in the denominator.

example, RC cases have a higher average union win rate and pro-union vote share than other petition types, as well as lower withdrawal rates. These level differences, though, are consistent over time. There is no sign that the effect of the rule change looks different when just considering RC cases. The same point holds for the additional analyses discussed in section II.

     I have only separately plotted RC cases here rather than, say, RD or RM cases. Non-RC petitions are comparatively rare, and show considerably more variability when graphed; but that variability is a function of the small sample size. Correspondingly, there are not significant differences in the patterns when considering these petition types.

References

Ferguson, John-Paul. 2009. Contest, Social Valuation and Change in American Labor-Union Organizing, 1961 to 2004. Unpublished PhD dissertation, Massachusetts Institute of Technology.

Ferguson, John-Paul 2018. Organizing Trends in CATS and NextGen NLRB Data. Technical report prepared for the AFL-CIO, January.
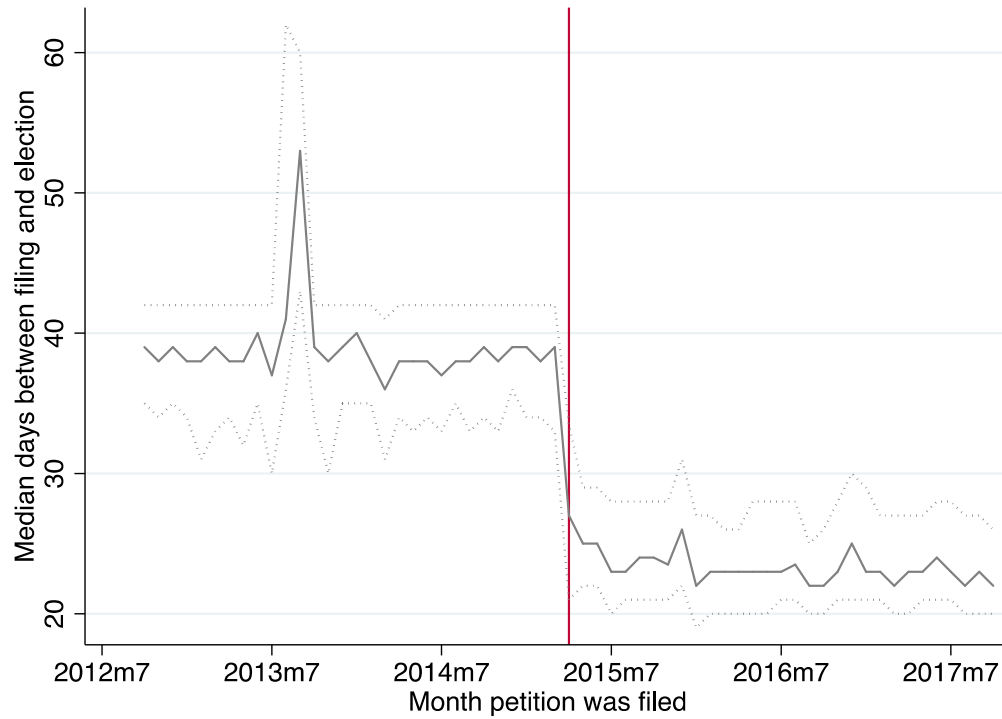
Figure 1: Median days between petition filing and election, October 2012 – October 2017, and interquartile range. The red line indicates the implementation of the election rule changes.
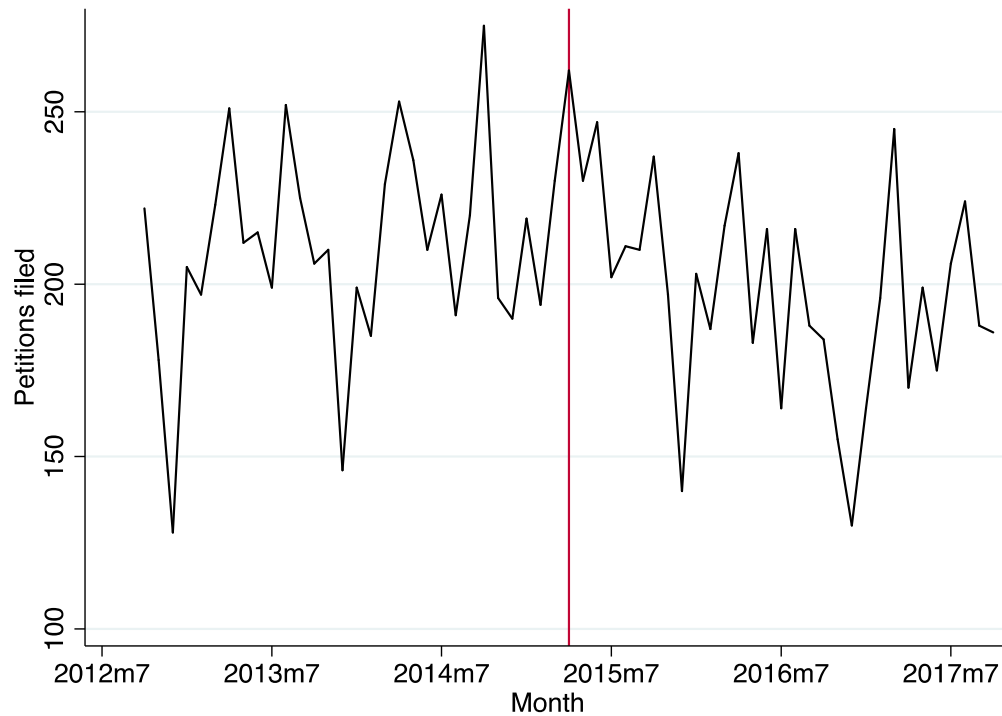
Figure 2: Monthly counts of petitions filed with the NLRB, October 2012 – October 2017. The red line indicates the implementation of the elections rule changes.
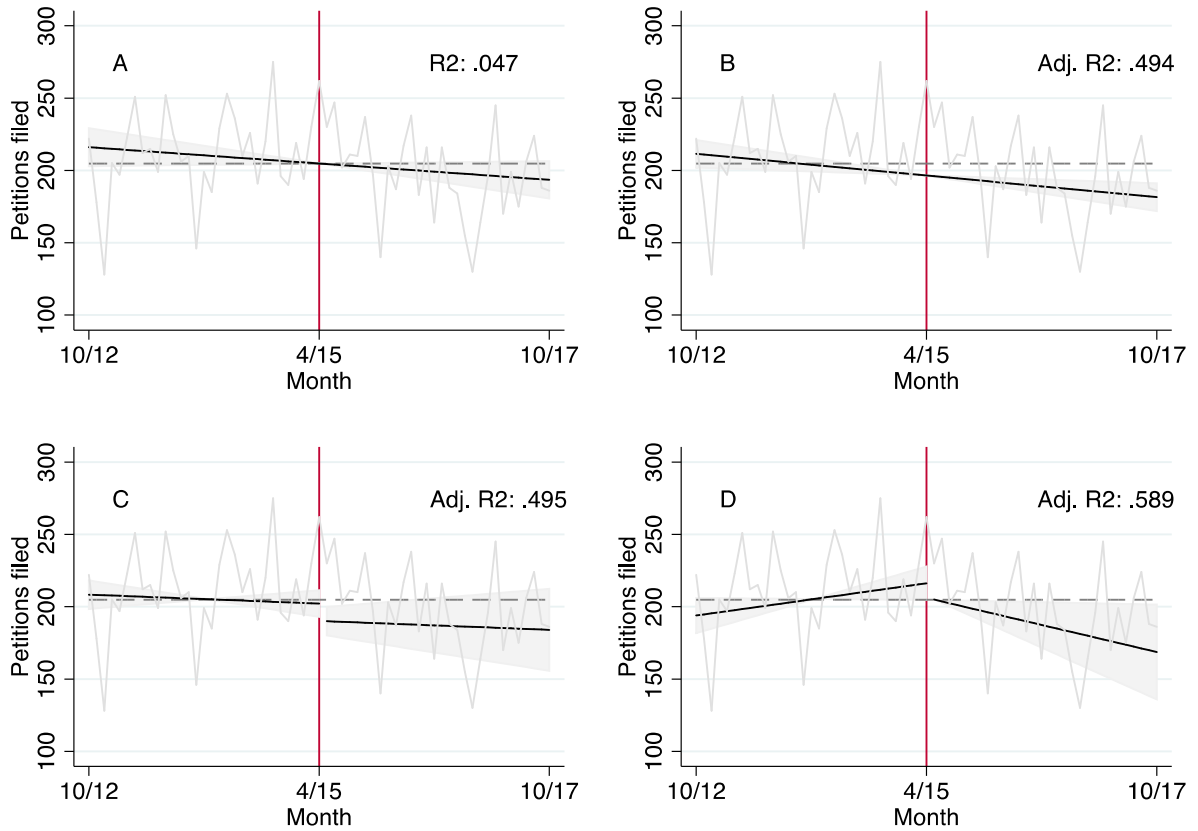
Figure 3: Estimating effects of the election rule changes on counts of petitions filed, with regression models. All plots show the raw data from figure 2 and superimpose estimated time trends (black solid lines), the uncertainty around those estimates (gray shading), and the zero-trend line (gray dashed line). Models report R^2, the proportion of variance explained; I present the adjusted R^2 for models with more than one independent variable. Panel A presents a linear regression of petition counts on time. Panel B includes indicator variables for calendar months. Panel C includes an indicator variable for the rule change in April 2015. Panel D interacts that rule change with the linear time trend. The red lines indicate the implementation of the elections rule changes.
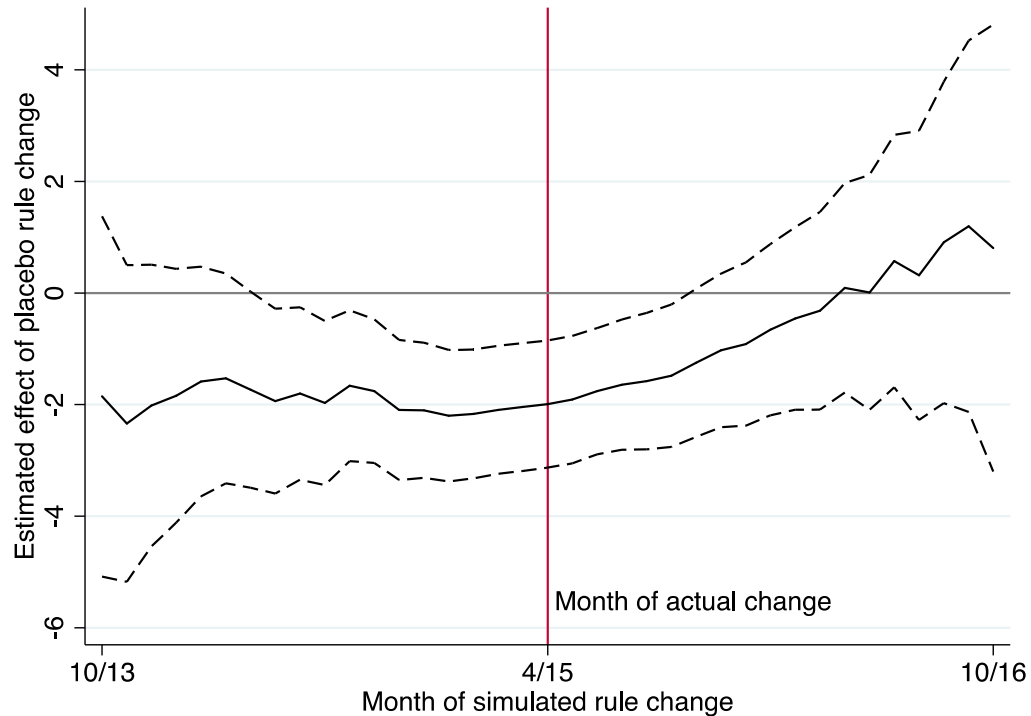
Figure 4: Placebo test for impact of the election rule changes on number of petitions filed per month. The red line indicates the implementation of the elections rule changes. The value of the solid and dashed lines where they cross the red line are the estimated coefficient and 95-percent confidence interval of the interaction between the rule change and the time trend of the model estimated in panel D of Figure 3. That is, the negative value here represents the negative slope of the post-change time trend in that earlier figure. I then fit 36 more models, varying the month that I used as the cutoff, ranging from 18 months before the actual rule change to 18 months after. Such models "detect" negative effects from the cutoff over roughly half of the time period. This should make us extremely skeptical that the effect found in figure 3 represents a real impact of the rule change.
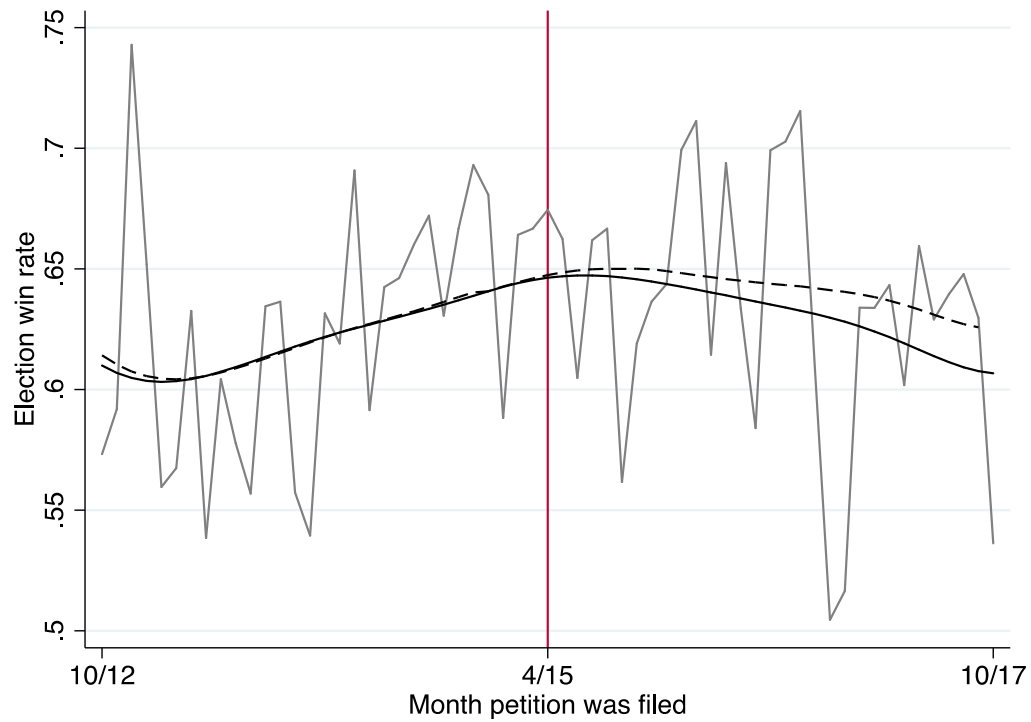
Figure 5: Average monthly union win rates in NLRB-supervised representation elections, October 2012 – October 2017. The red line marks the implementation of the election rule changes. The solid black line is a LOWESS curve; see the text for details. The dashed black line is also a LOWESS curve, estimated while excluding petitions filed in October and November 2016 from the analysis. The post-rule time trend of the dashed line is indistinguishable from zero.
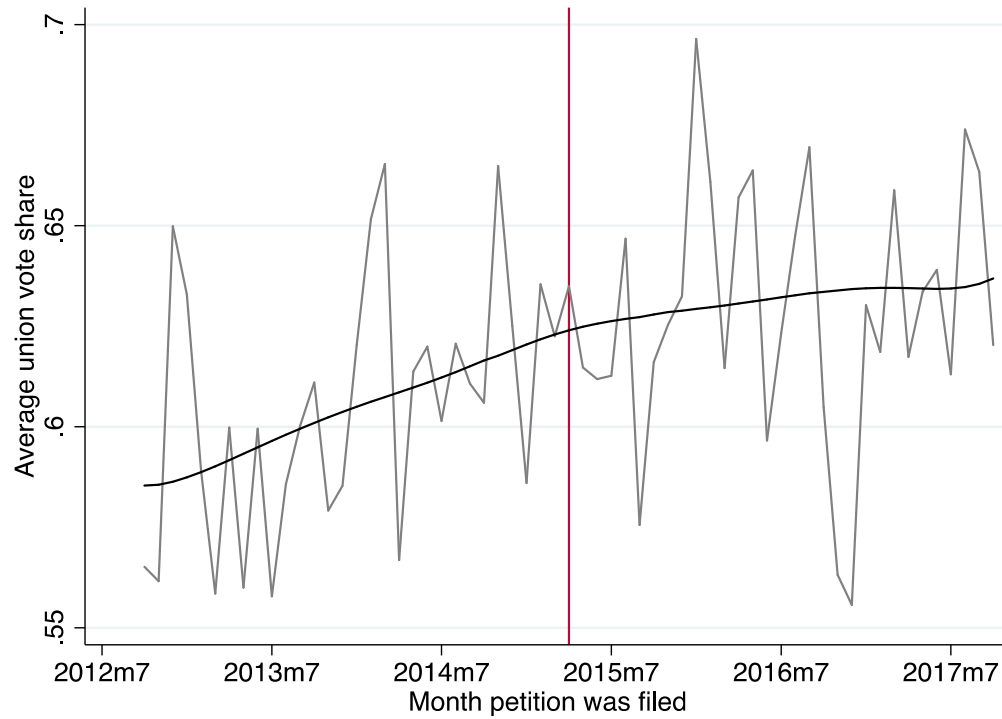
Figure 6: Average union vote share in representation elections, October 2012 – October 2017. Red line indicates implementation of the election rule changes. Differences in the positive time trend before and after the rule change are not statistically significant.
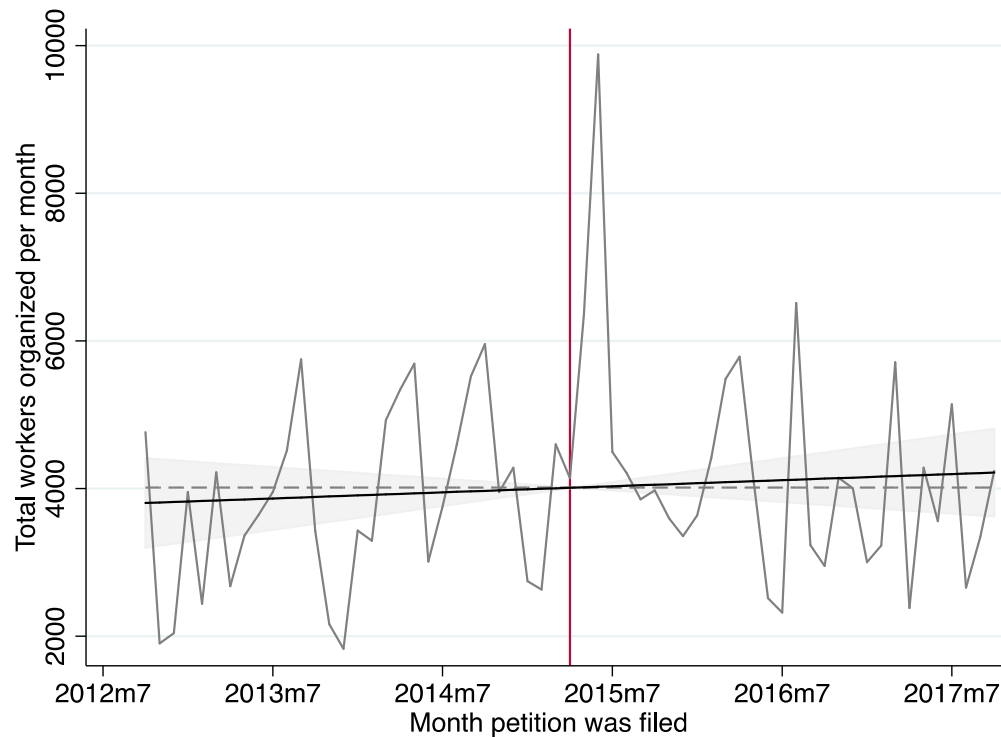
Figure 7: Total workers gaining representation per month, October 2012 – October 2017. Red line indicates implementation of the election rule changes. The spike in workers gaining representation in May 2015 obviously merits further investigation, but it does not alter the fact that the estimated time trend for workers added during this period (black line) is not statistically distinguishable from zero (grey dashed line).
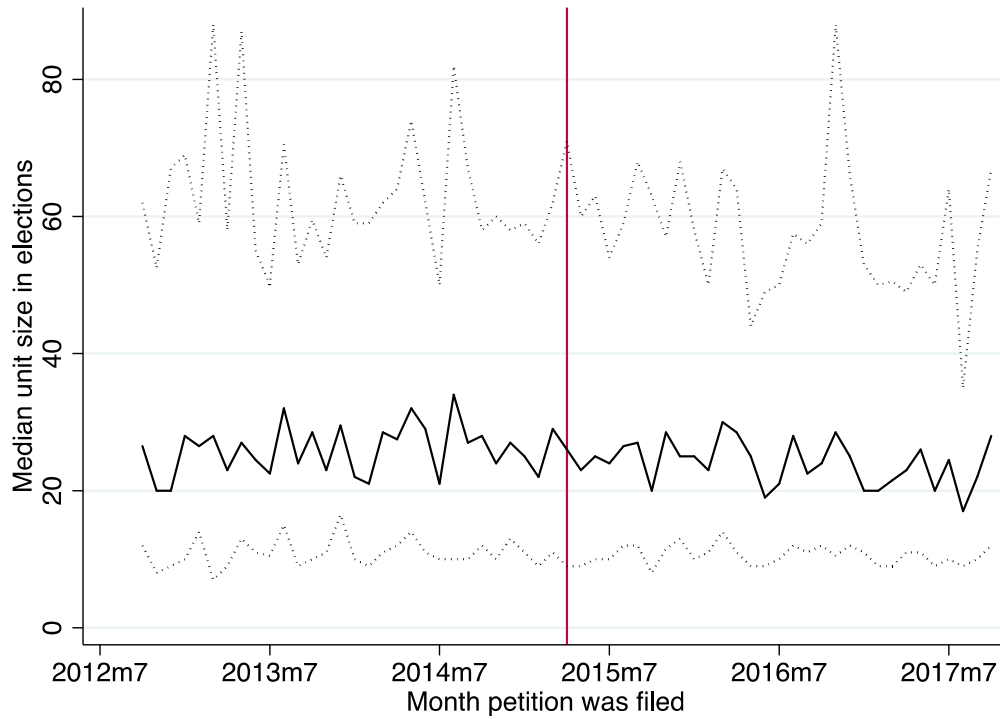
Figure 8: Median unit size in elections with interquartile range, October 2012 – October 2017. The red line represents the implementation of the election rule changes. There is a statistically significant downward trend in the median unit size, but it is substantively trivial (a reduction of just .05 workers per month) and unrelated to the rule change.
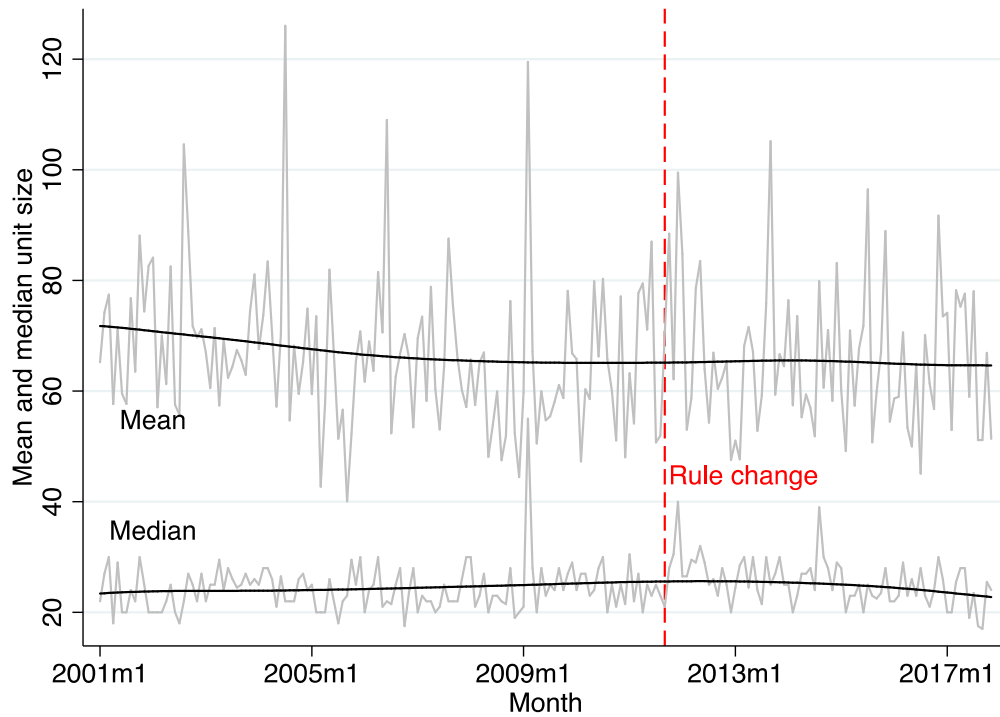
Figure 9: Mean and median unit size with LOWESS fitted curves, 2001 – 2017. This figure combines information from representation cases in the NLRB's CATS and NxGen case-tracking systems. The rule change flagged in red pertains to the Board's decision in *Specialty Healthcare & Rehabilitation Center of Mobile*, 357 NLRB 934 (2011), issued on August 26, 2011. There is no change in either mean or median unit size in the wake of this promulgation.
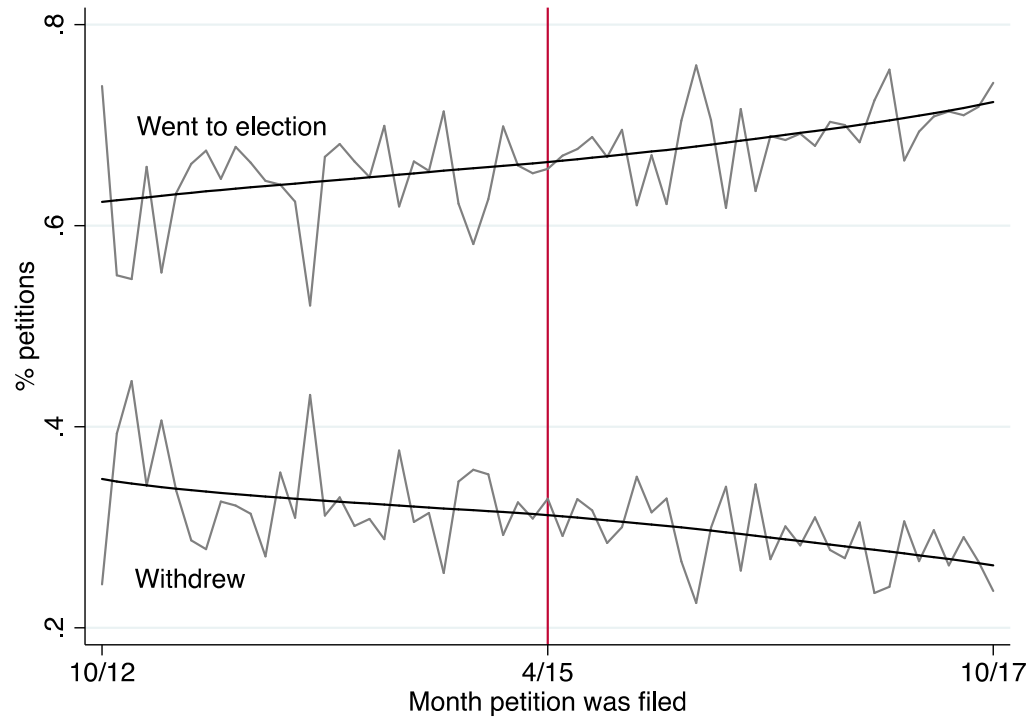
Figure 10: Rate of monthly petition filings that result in election or withdrawal, October 2012 – October 2017. The raw data are in gray. Black lines are LOWESS curves; red line indicates implementation of the elections rule changes. LOWESS curves are non-linear but in this case the local trend is constant over nearly the entire range.
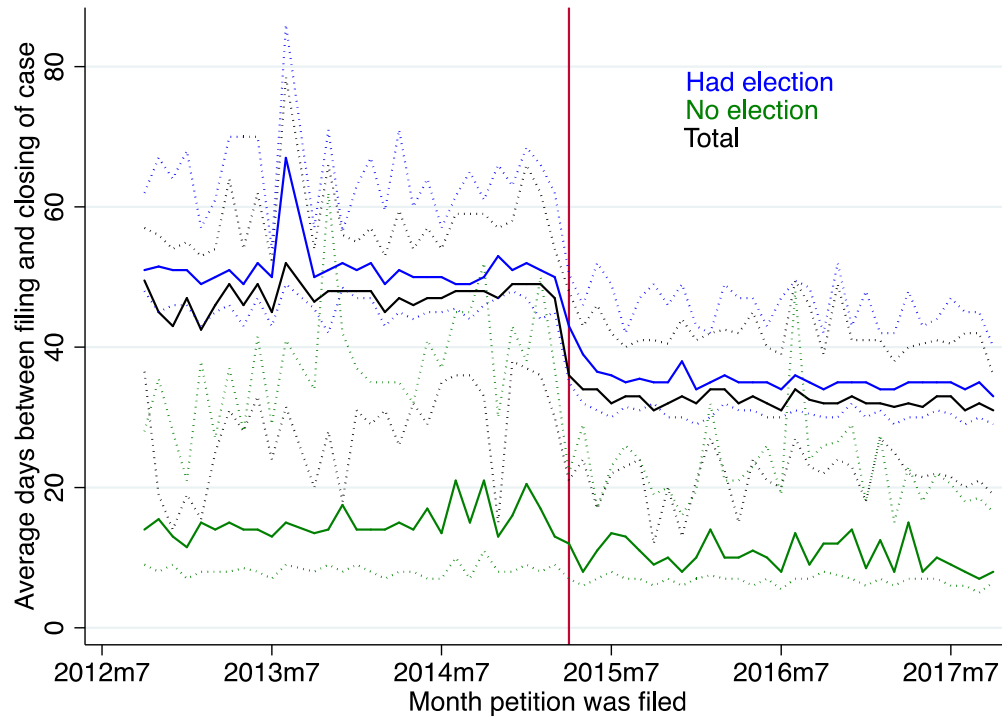
Figure 11: Median case lengths in days; dotted lines show interquartile ranges. The red line marks the implementation of the election rule changes. Most of the decline in case length in the wake of the election rule occurred among cases that held elections, but the length of other cases also declined. This difference is statistically significant.
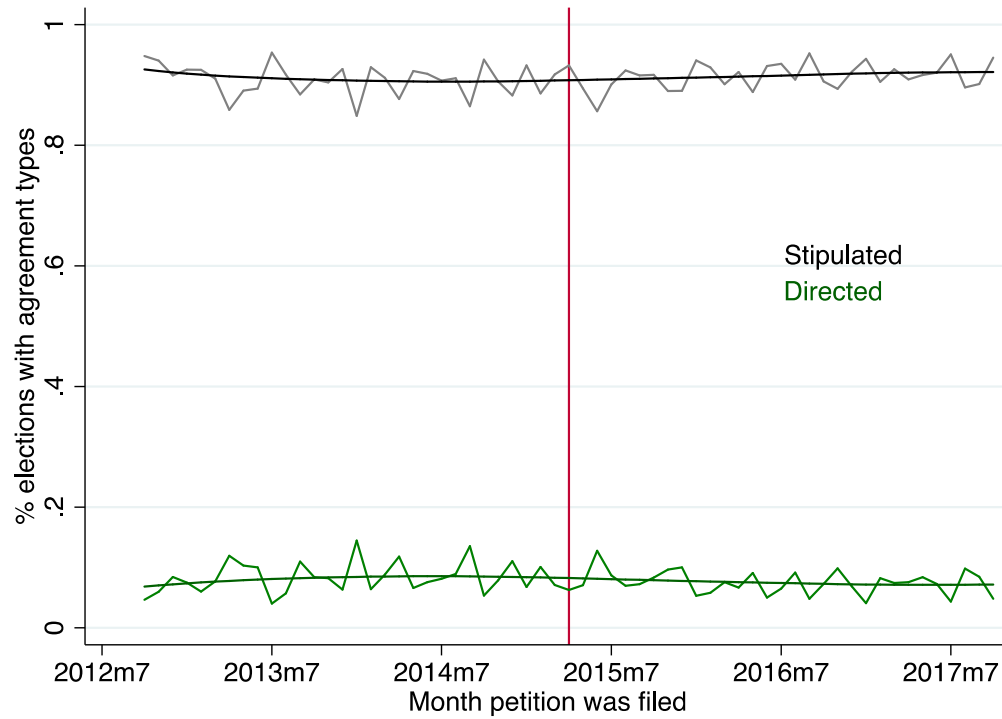
Figure 12: Rate of stipulated and directed elections in monthly filings, October 2012 – October 2017. The red line marks the implementation of the election rule changes.
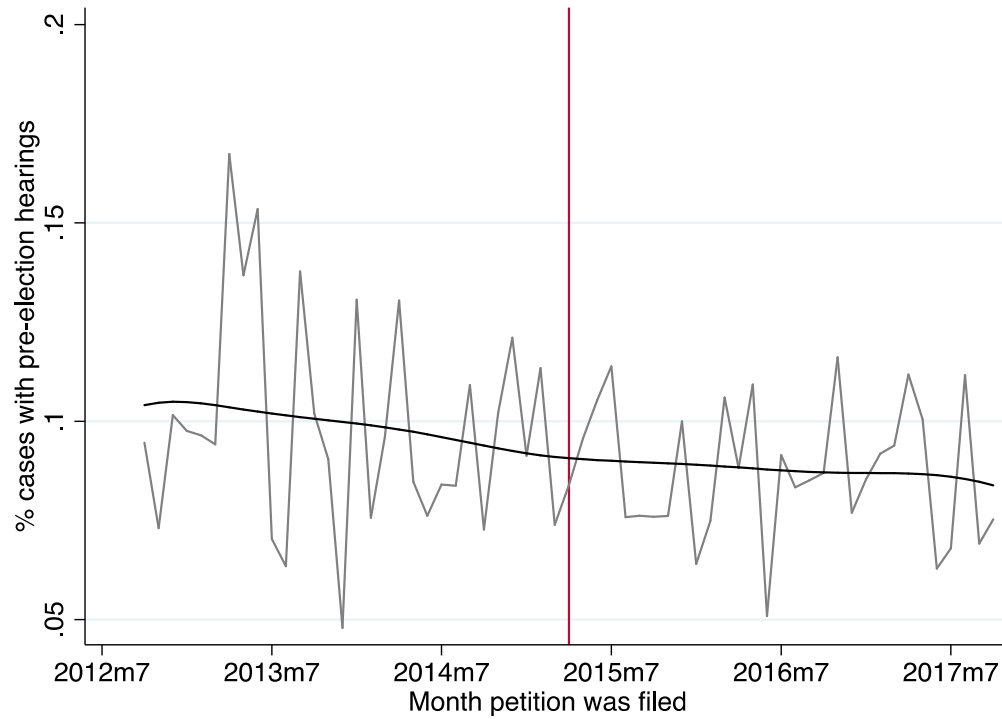
Figure 13: Average rate of monthly filings leading to pre-election hearings, October 2012 – October 2017. The plotted time trend is indistinguishable from zero. The red line marks the implementation of the election rule changes.
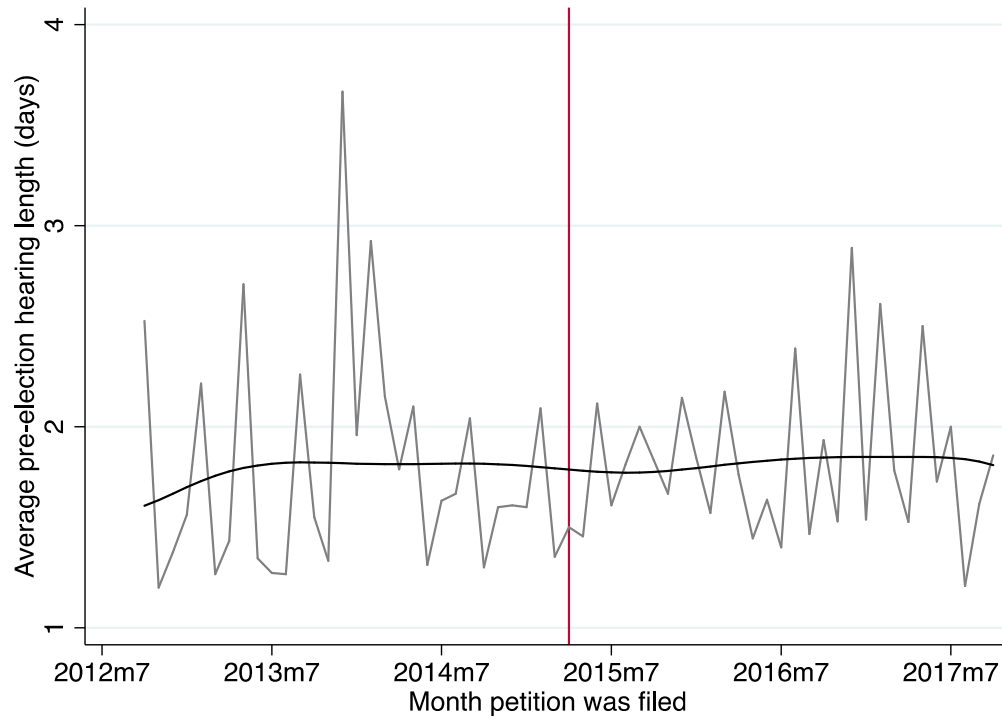
Figure 14: Average length of pre-election hearings, in days, October 2012 – October 2017. The red line marks the implementation of the election rule changes.
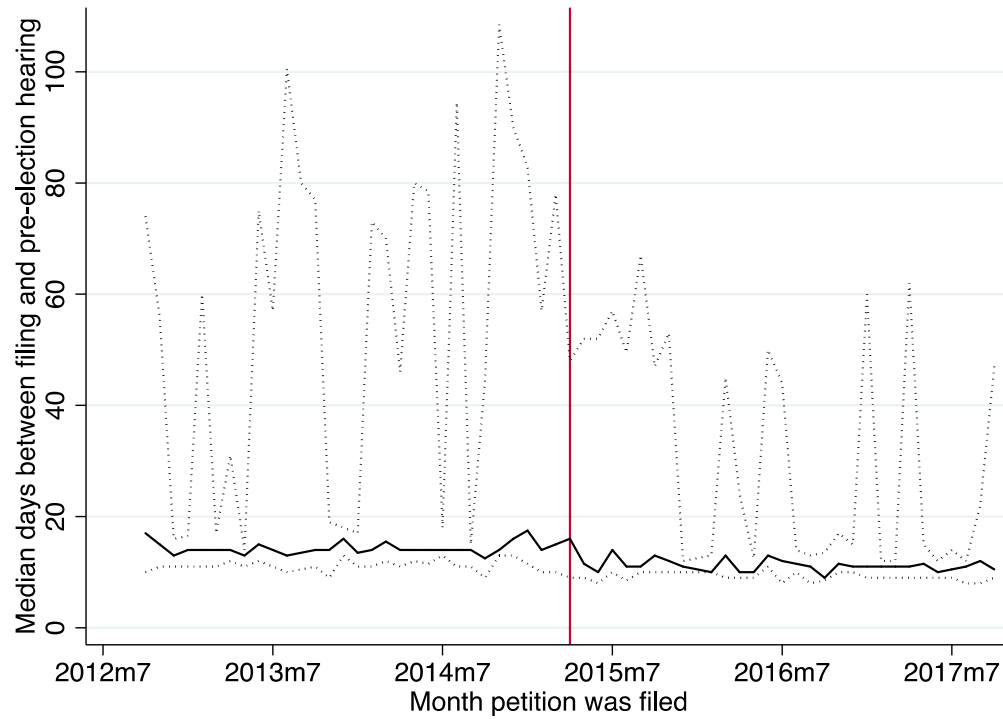
Figure 15: Median days between filing and pre-election hearing with interquartile range, October 2012 – October 2017. Decline in median days after the rule change (red line) is statistically significant.
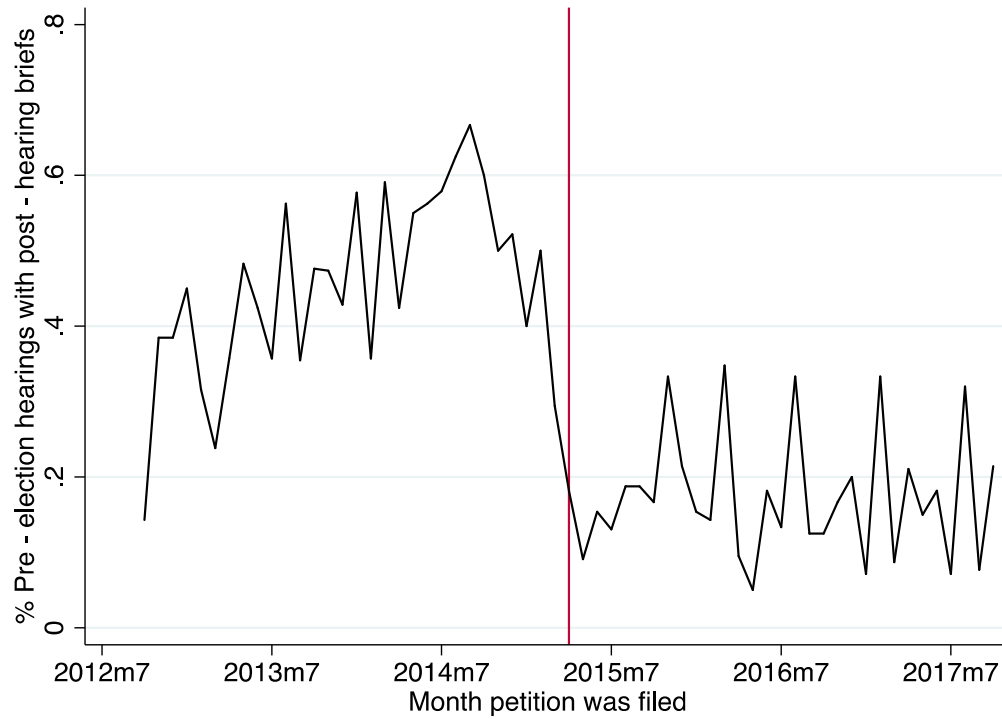
Figure 16: Percentage of those cases in which pre-election hearing was held in which briefs were filed by any party. The red line marks the implementation of the election rule changes.
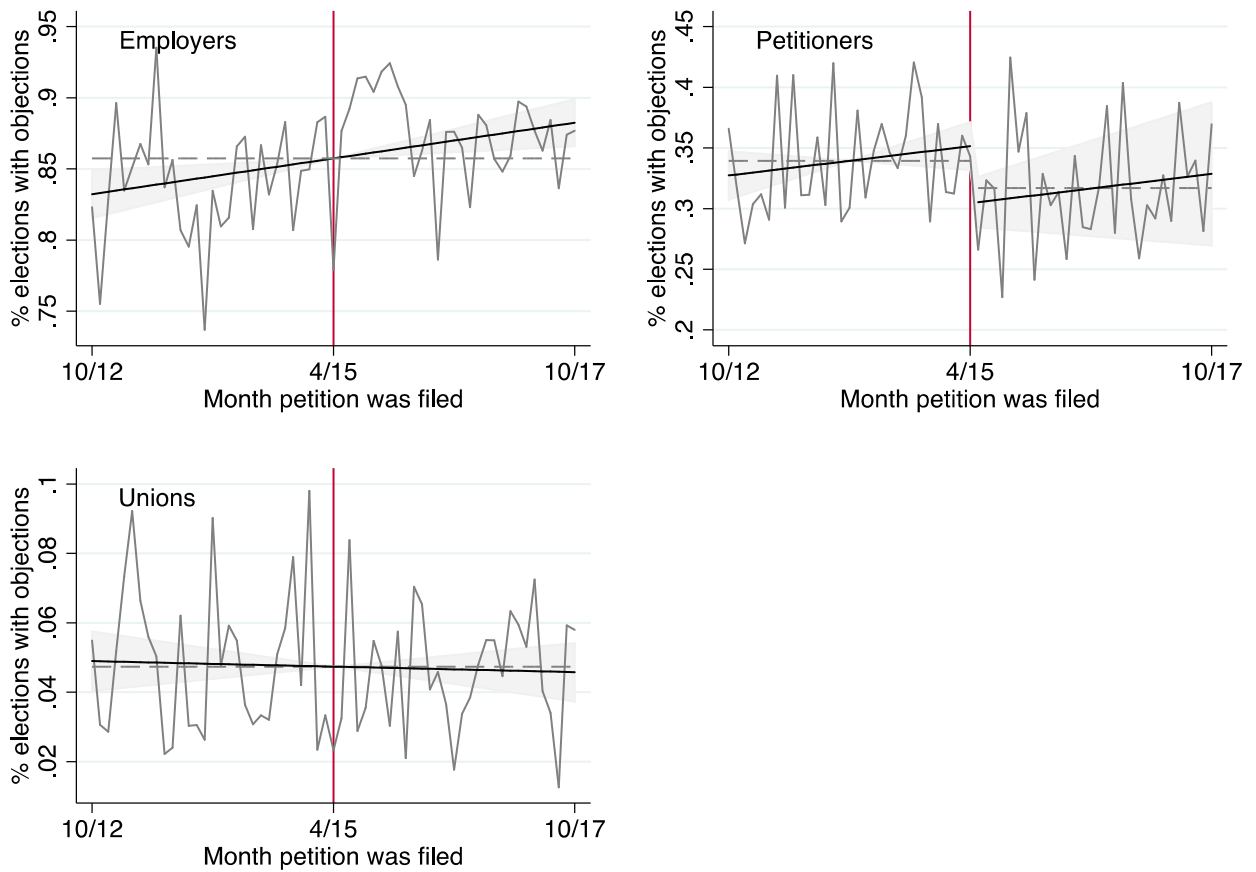
Figure 17: Rates of objections filed after representation elections, October 2012 – October 2017, by filing party. Note the different vertical scales. The employer objection filing rate has risen with time, but shows no relationship with the rule change. The petitioner objection filing rate does not have a statistically significant time trend, but the means before and after the rule change are significantly different. The union objection filing rate shows no temporal trend. The red line marks the implementation of the election rule changes.
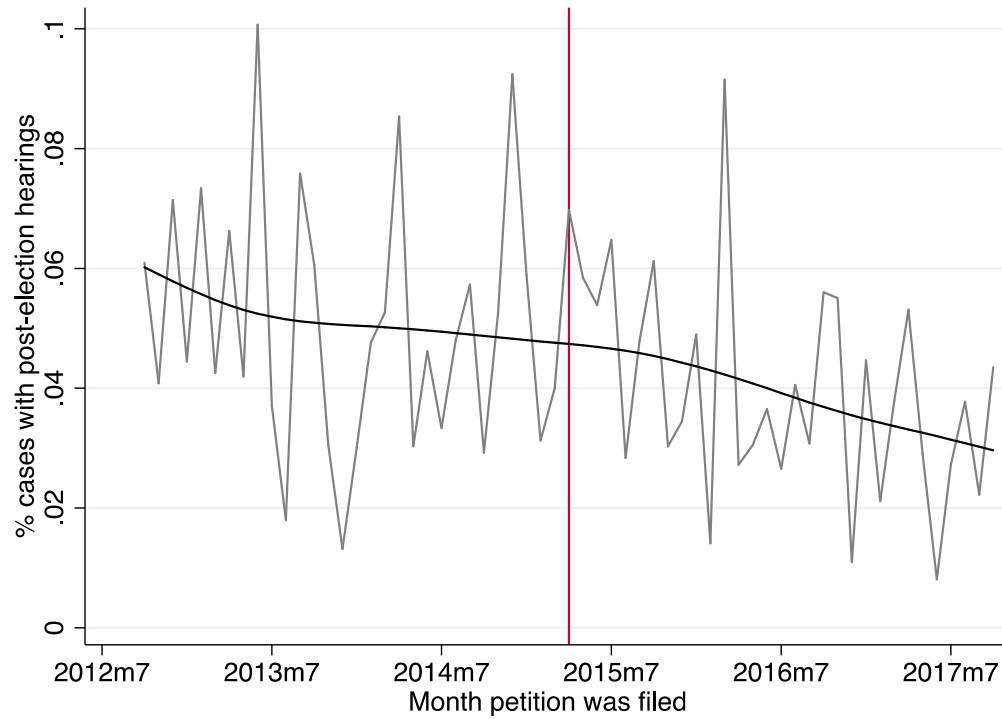
Figure 18: Share of cases that went to election that had post-election hearings, October 2012 – October 2017. Red line indicates implementation of the election rule changes. Downward trend is uncorrelated with the rule change.
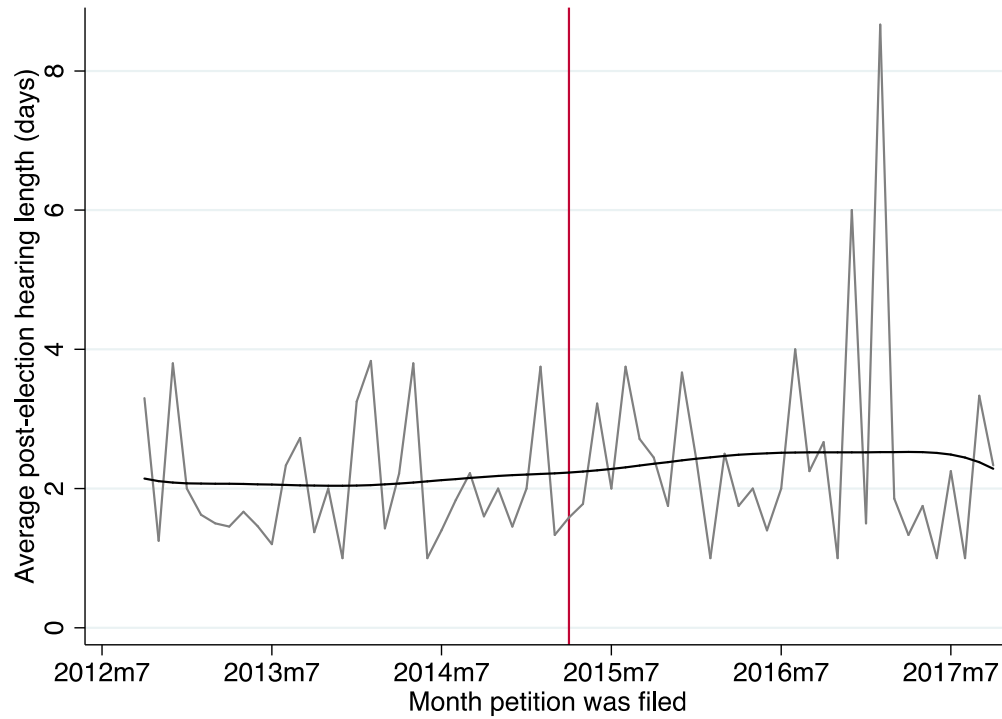
Figure 19: Typical post-election hearing lengths, in days, October 2012 – October 2017. Red line indicates implementation of the election rule changes. No significant change is associated with the rule changes.
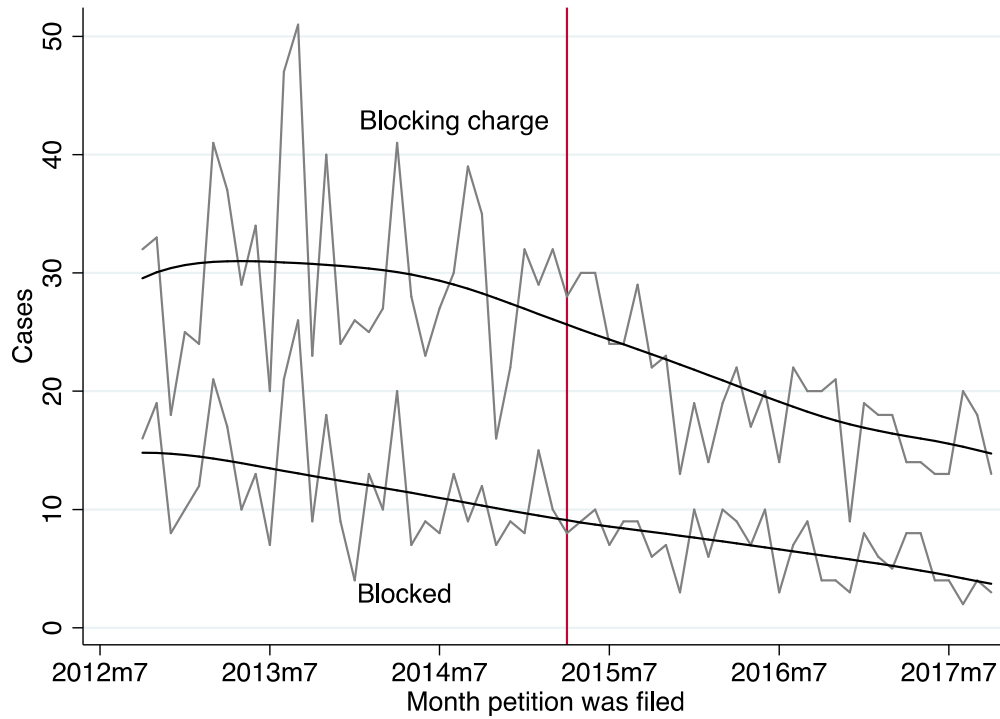
Figure 20: Numbers of blocking charges filed per month and number of cases blocked, October 2012 – October 2017. Cases blocked as a share of blocking charges is nearly constant over time. Red line indicates implementation of the election rule changes.
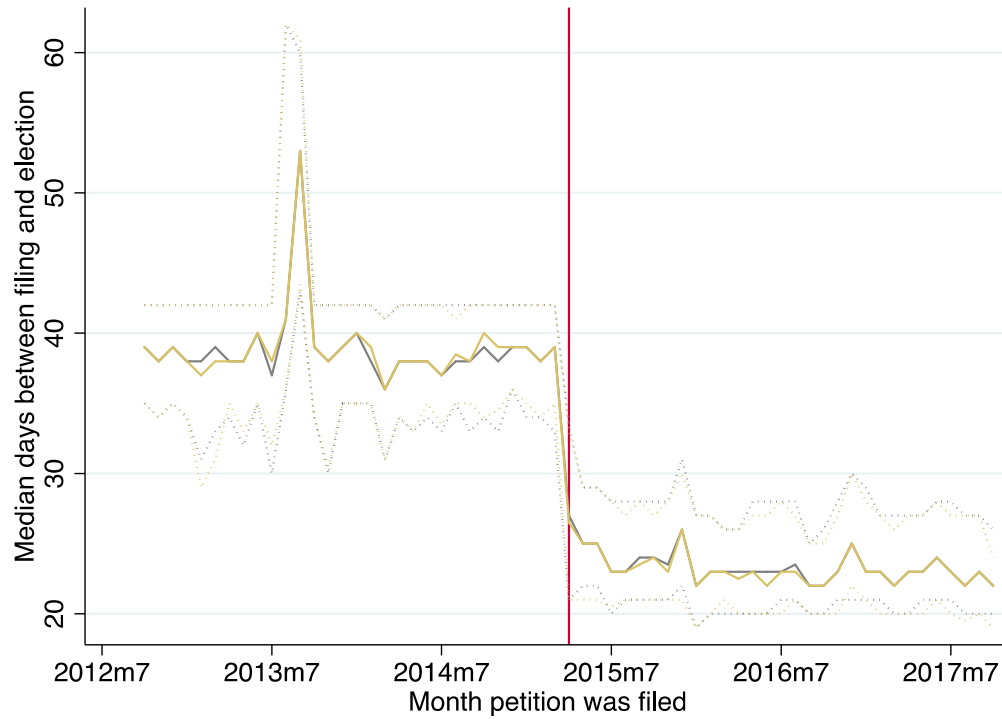
Figure 1A: Median days between petition filing and election, October 2012 – October 2017. The black line reproduces figure 1; the tan overlay, which almost perfectly covers the black line, is calculated only for RC cases. Dotted lines show the interquartile range. The red line indicates the implementation of the election rule changes.
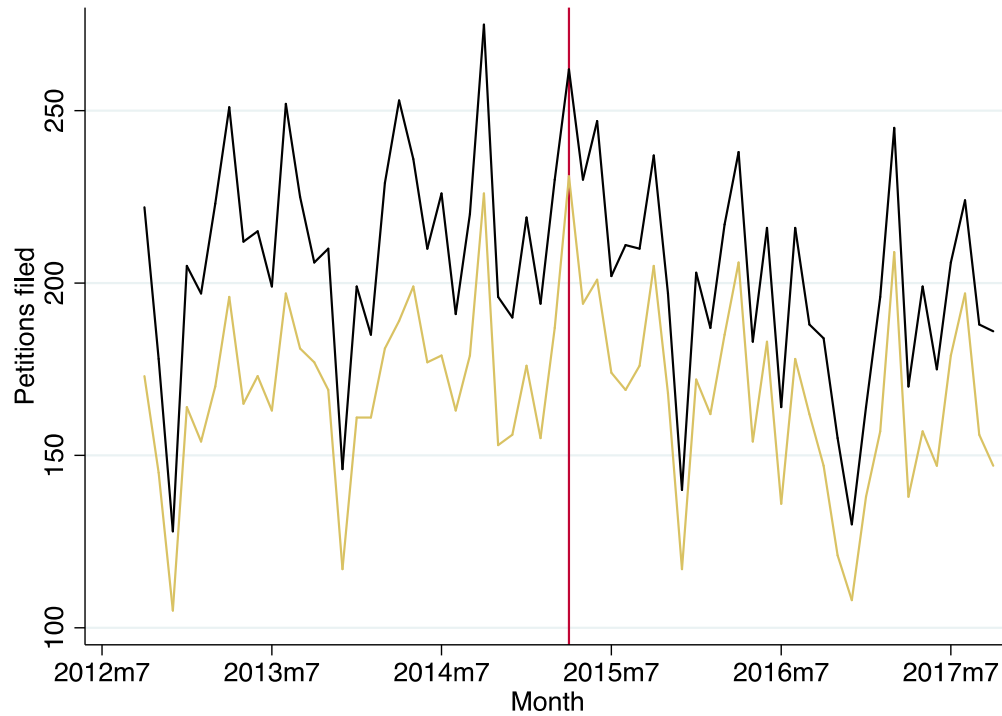
Figure 2A: Monthly counts of petitions filed with the NLRB, October 2012 – October 2017. The black line reproduces figure 2; the tan overlay is calculated only for RC cases. The red line indicates the implementation of the elections rule changes.
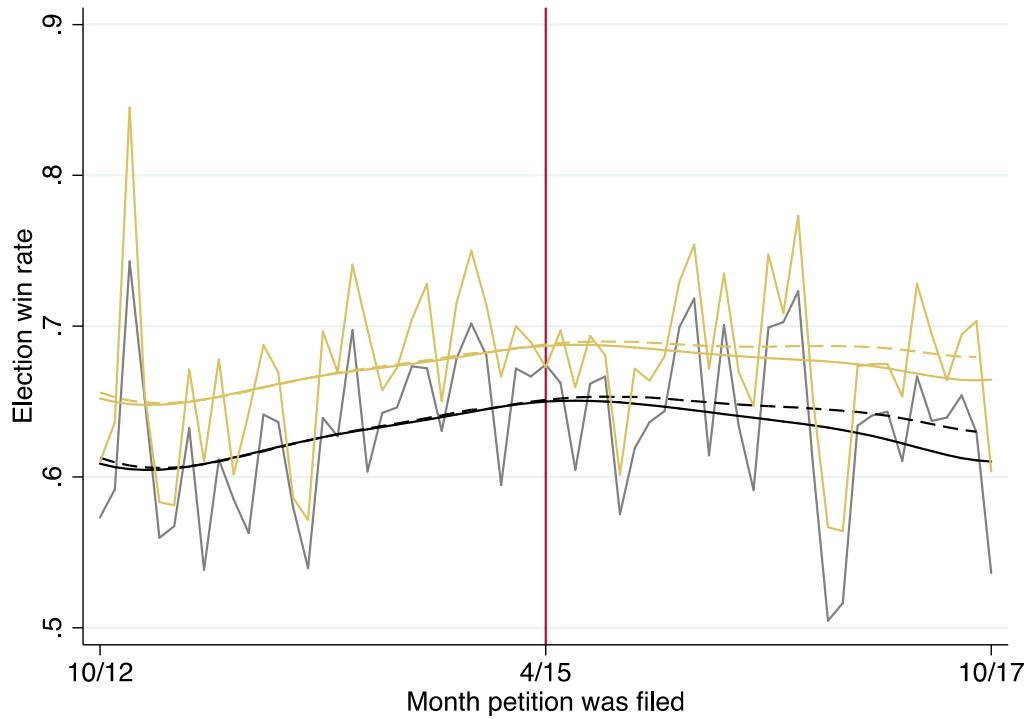
Figure 5A: Average monthly union win rates in NLRB-supervised representation elections, October 2012 – October 2017. The black line reproduces figure 3; the tan overlay is calculated only for RC cases. The red line marks the implementation of the election rule changes. The solid black line is a LOWESS curve; see the text for details. The dashed black line is also a LOWESS curve, estimated while excluding petitions filed in October and November 2016 from the analysis. The post-rule time trend of the dashed line is indistinguishable from zero.
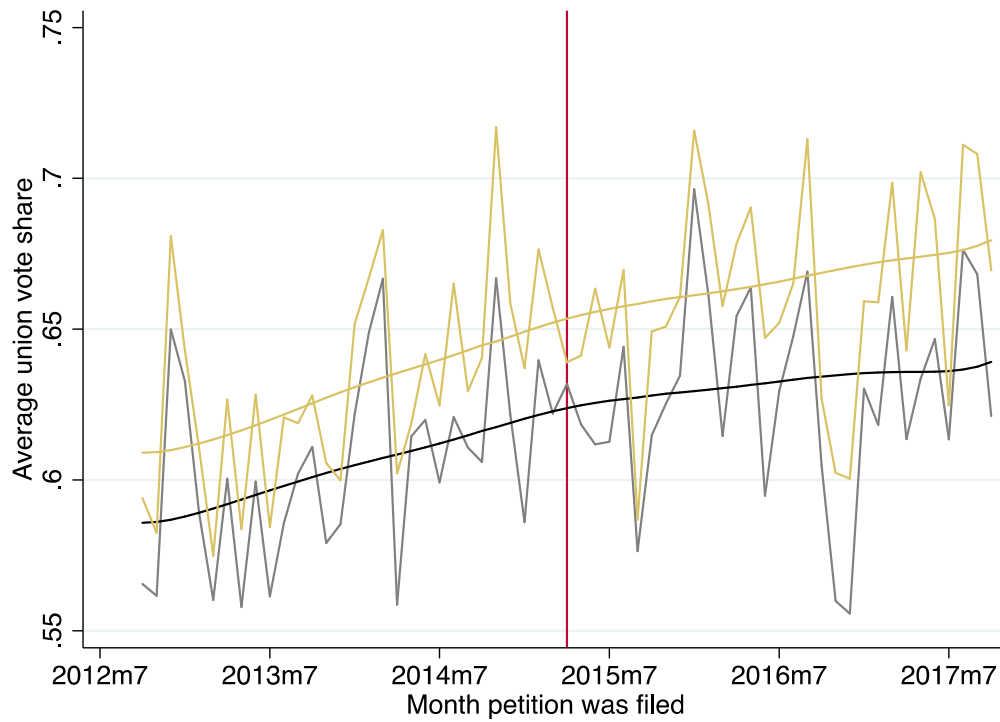
Figure 6A: Average union vote share in representation elections, October 2012 – October 2017. The black line reproduces figure 6; the tan overlay is calculated only for RC cases. Red line indicates implementation of the election rule changes. Differences in the positive time trend before and after the rule change are not statistically significant.
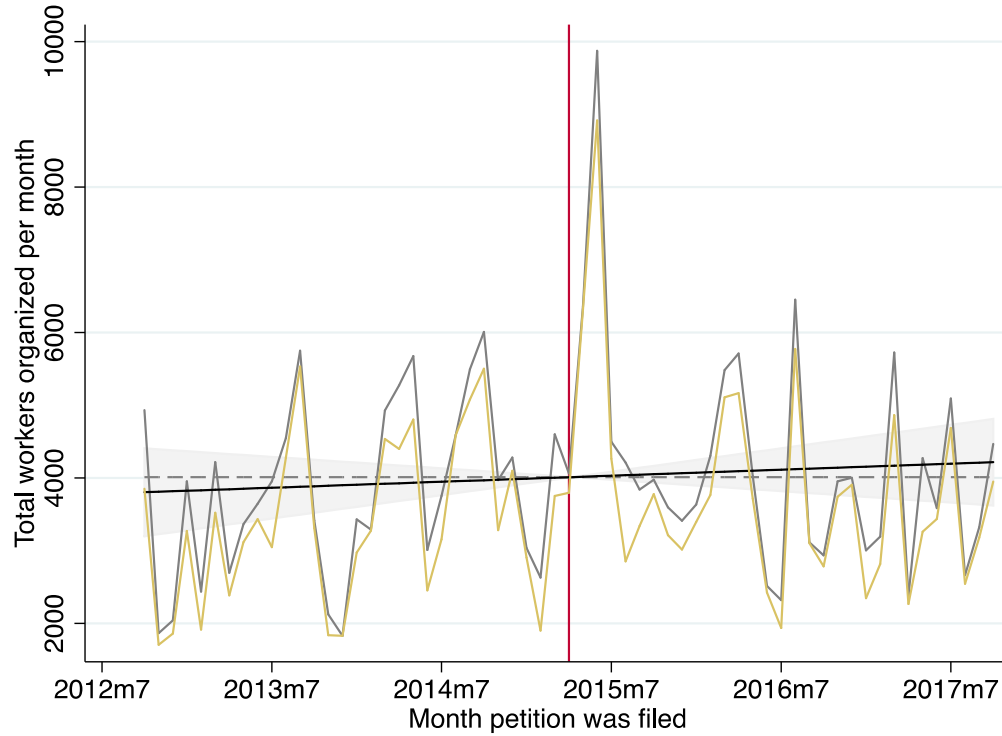
Figure 7A: Total workers gaining representation per month, October 2012 – October 2017. The black line reproduces figure 7; the tan overlay is calculated only for RC cases. Red line indicates implementation of the election rule changes. The spike in workers gaining representation in May 2015 obviously merits further investigation, but it does not alter the fact that the estimated time trend for workers added during this period (black line) is not statistically distinguishable from zero (grey dashed line).
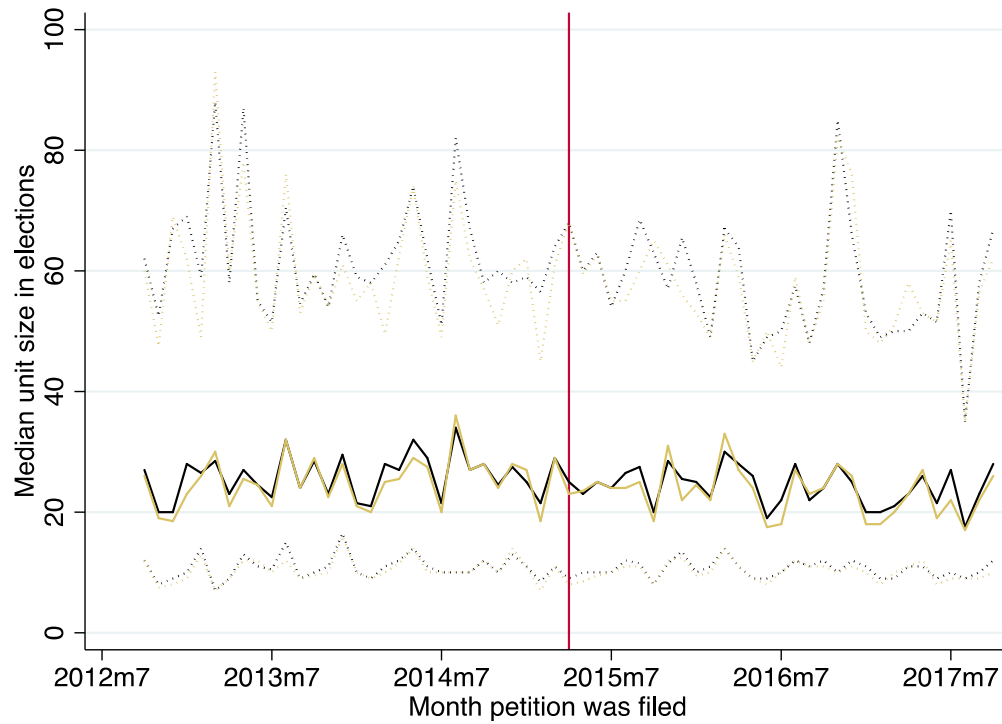
Figure 8A: Median unit size in elections, October 2012 – October 2017. The black line reproduces figure 8; the tan overlay is calculated only for RC cases. Dotted lines show the interquartile range. The red line represents the implementation of the election rule changes. There is a statistically significant downward trend in the median unit size, but it is substantively trivial (a reduction of just .05 workers per month) and unrelated to the rule change.
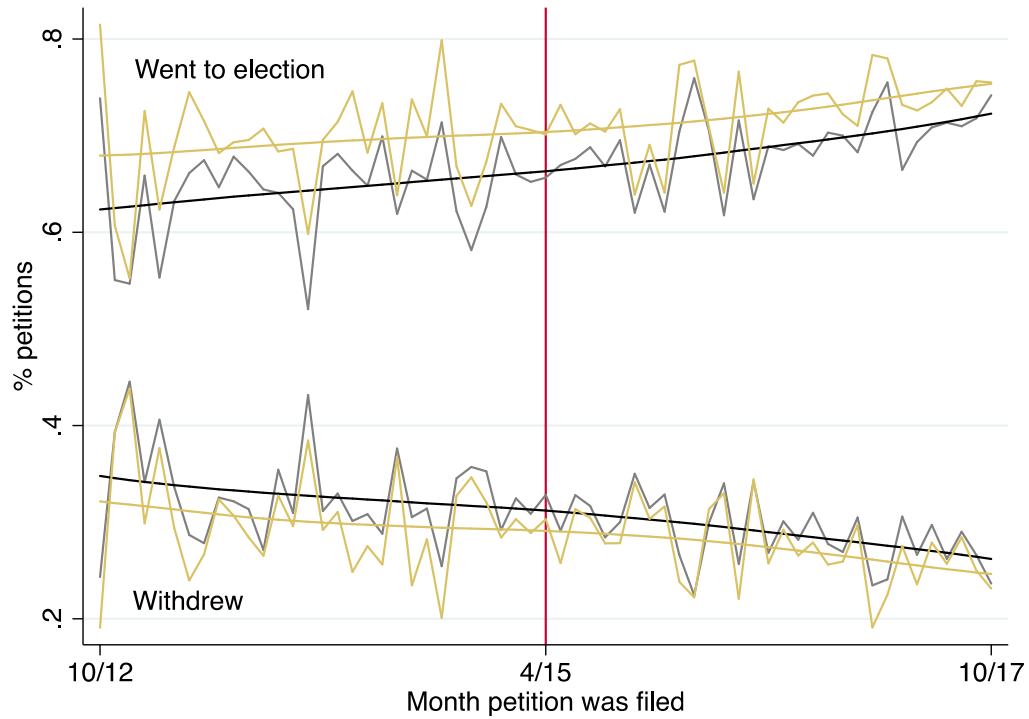
Figure 10A: Rate of monthly petition filings that result in election or withdrawal, October 2012 – October 2017. The gray and black lines reproduce figure 10; the tan overlay is calculated only for RC cases. The smooth lines are LOWESS curves; the red line indicates implementation of the elections rule changes. LOWESS curves are non-linear but in this case the local trend is constant over nearly the entire range.
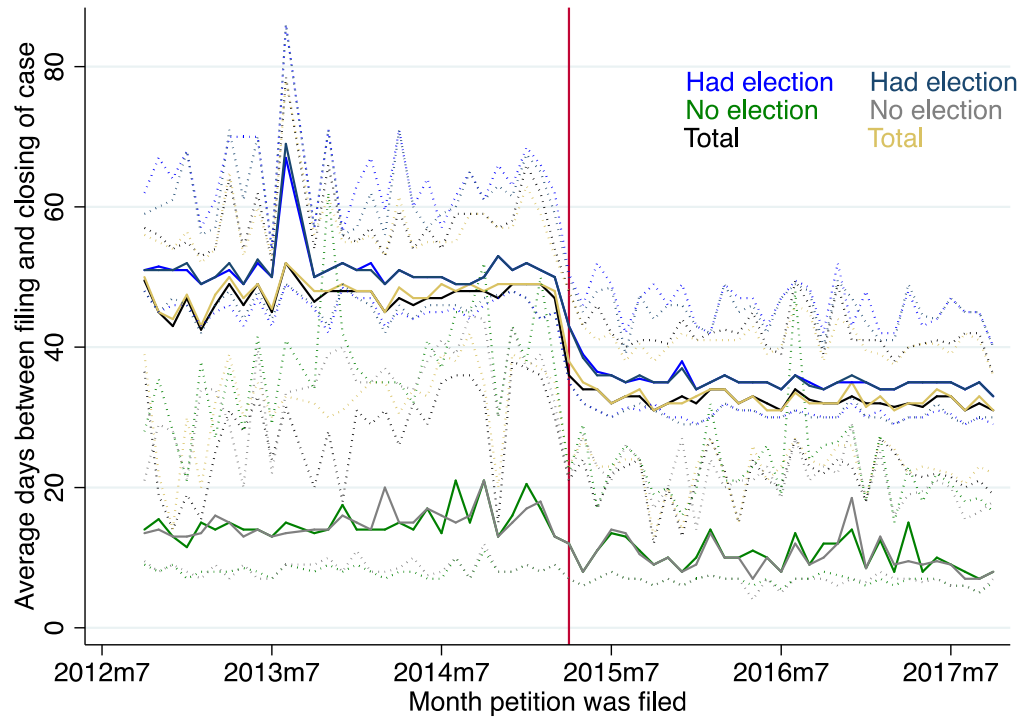
Figure 11A: Median case lengths in days. The blue, green, and black lines reproduce figure 11; the navy, grey and tan overlays are calculated only for RC cases. Correspondingly colored dotted lines show the interquartile ranges. The red line marks the implementation of the election rule changes. Most of the decline in case length in the wake of the election rule amendments occurred among cases that held elections, but the length of other cases also declined. This difference is statistically significant.
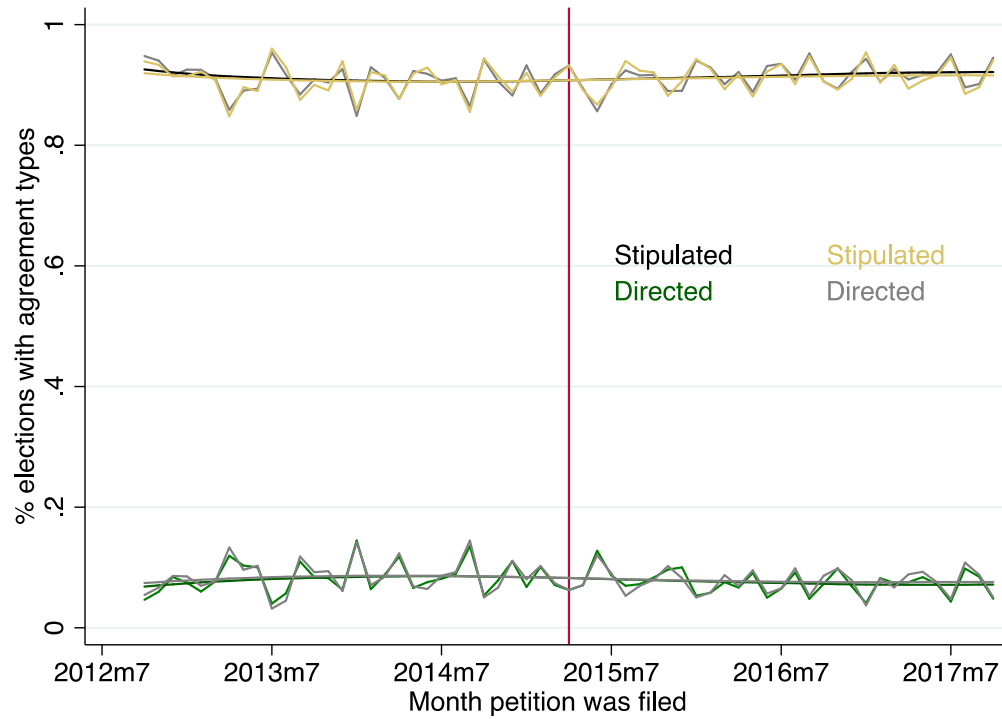
Figure 12A: Rate of stipulated and directed elections in monthly filings, October 2012 – October 2017. The black and green lines reproduce figure 12; the tan and gray overlays are calculated only for RC cases. The red line marks the implementation of the election rule changes.
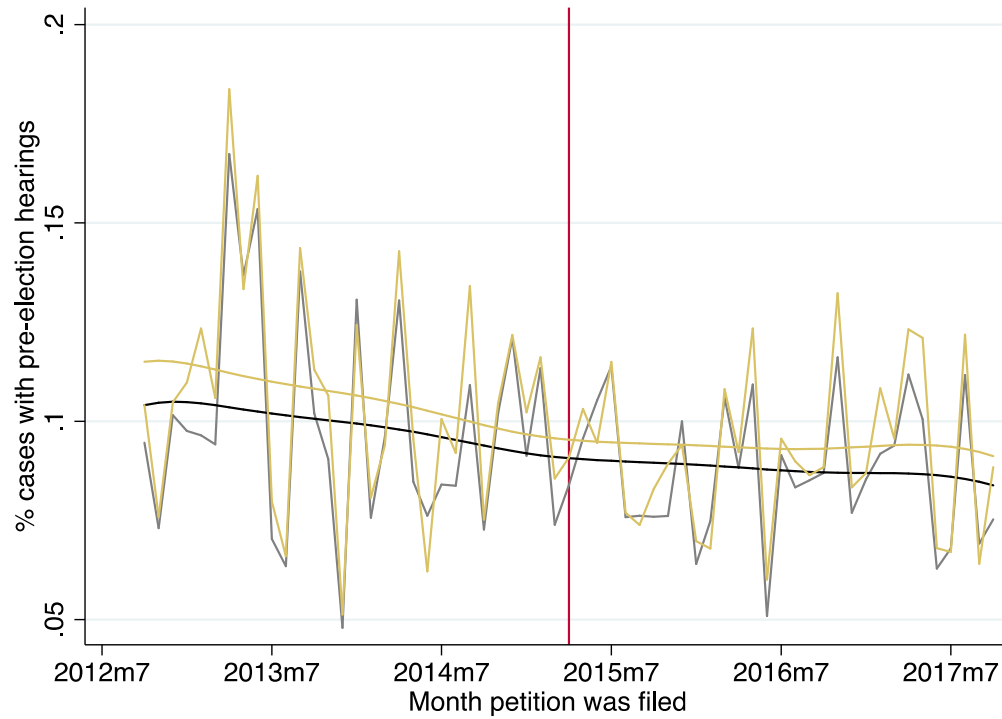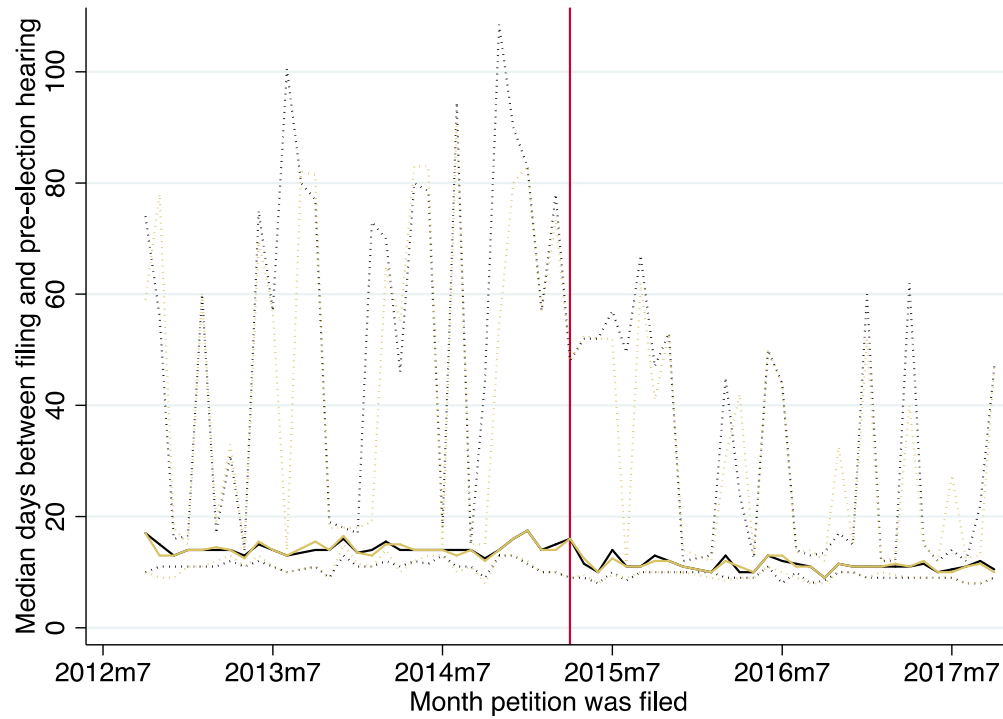
Figure 13A: Average rate of monthly filings leading to pre-election hearings, October 2012 – October 2017. The black line reproduces figure 13; the tan overlay is calculated only for RC cases. The plotted time trend is indistinguishable from zero. The red line marks the implementation of the election rule changes.
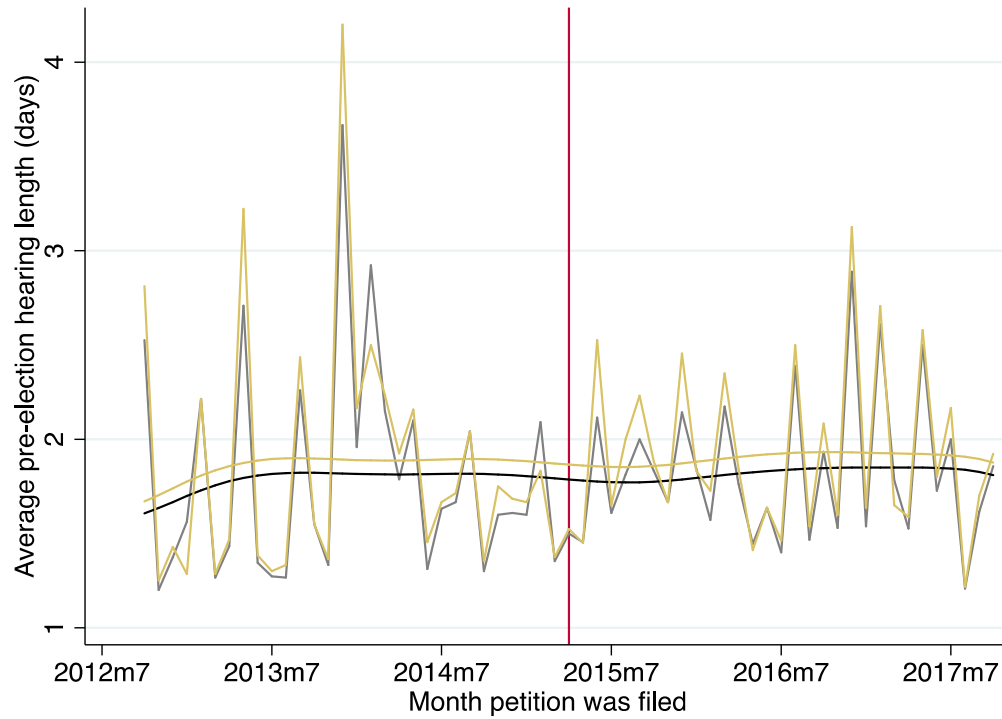
Figure 14A: Average length of pre-election hearings, in days, October 2012 – October 2017. The black line reproduces figure 1; the tan overlay is calculated only for RC cases. The red line marks the implementation of the election rule changes.
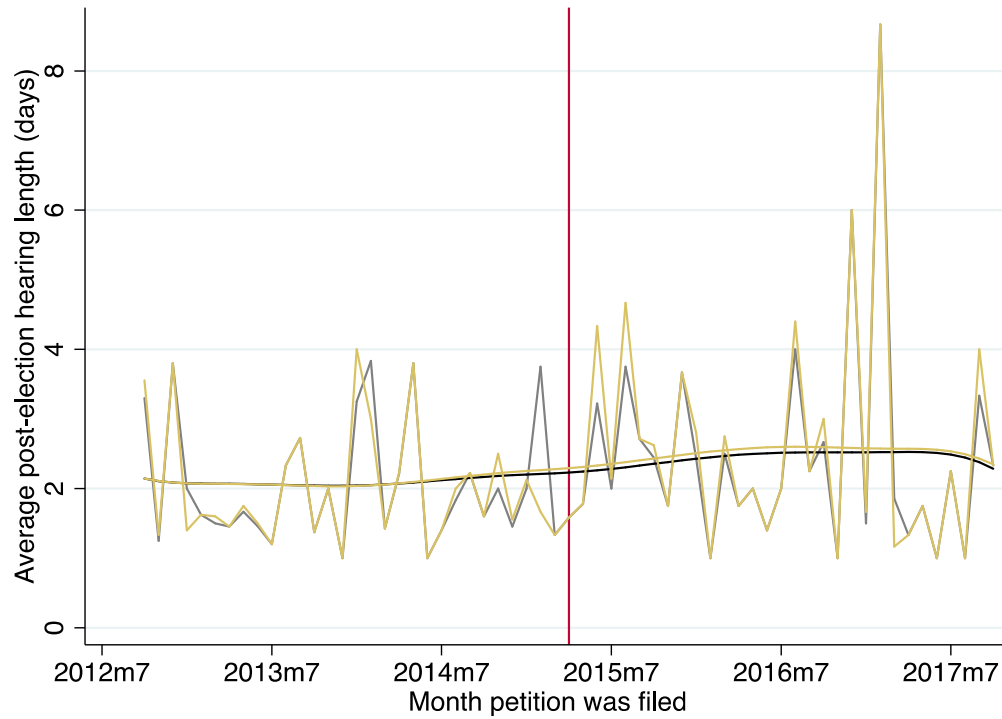
Figure 15A: Median days between filing and pre-election hearing, October 2012 – October 2017. The black line reproduces figure 1; the tan overlay is calculated only for RC cases. Correspondingly colored dotted lines indicate the interquartile range. Decline in median days after the rule change (red line) is statistically significant.
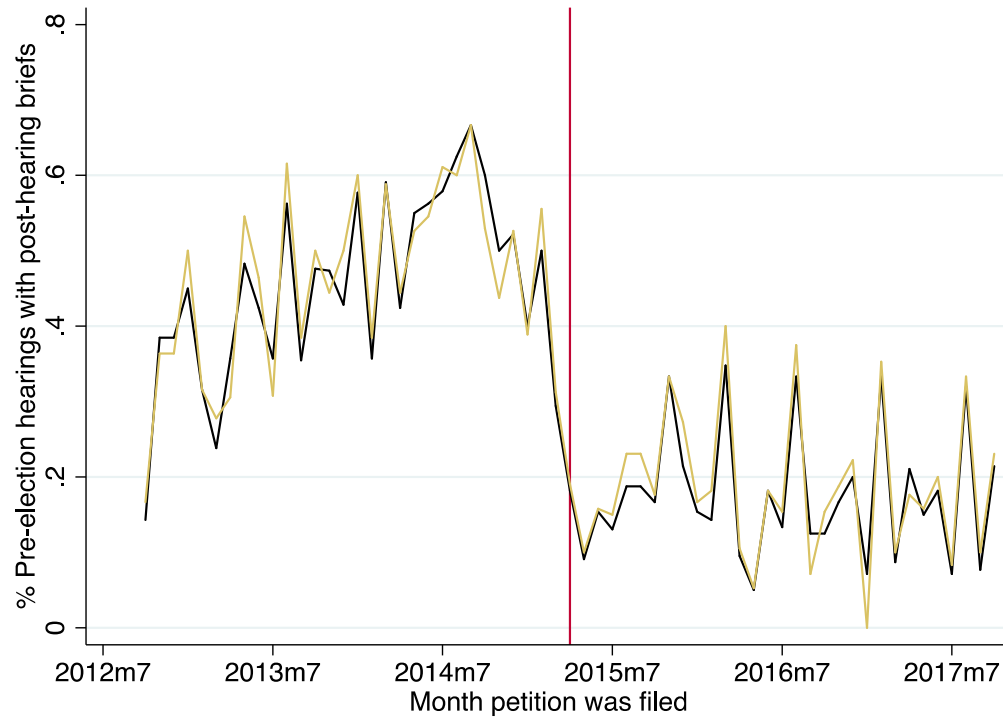
Figure 16A: Percentage of those cases in which pre-election hearing was held in which briefs were filed by any party. The black line reproduces figure 16; the tan line is calculated solely on RC cases. The red line marks the implementation of the election rule changes.

Figure 18A: Share of cases that went to election that had post-election hearings, October 2012 – October 2017. In this case, post-election hearings coincided completely with RC cases. Red line indicates implementation of the election rule changes. Downward trend is uncorrelated with the rule change.

Figure 19A: Typical post-election hearing lengths, in days, October 2012 – October 2017. The black line reproduces figure 19; the tan overlay is calculated only for RC cases. Red line indicates implementation of the election rule changes. There is not significant change associated with the rule changes.
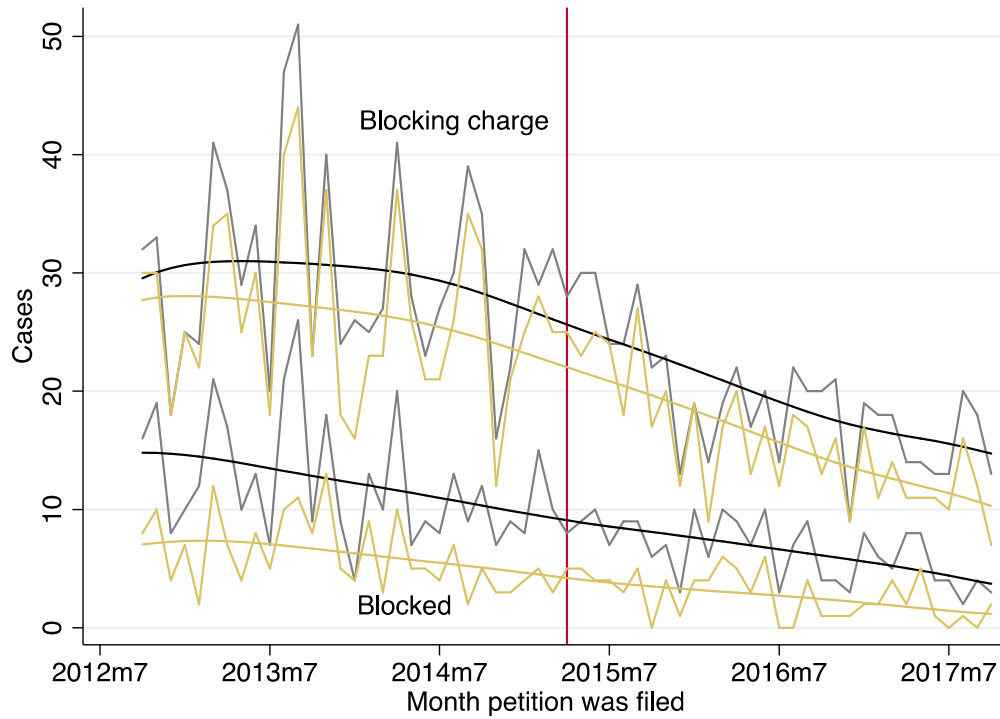
Figure 20A: Numbers of blocking charges filed per month and number of cases blocked, October 2012 – October 2017. The black lines reproduce figure 20; the tan overlays are calculated only for RC cases. Cases blocked as a share of blocking charges is nearly constant over time. Red line indicates implementation of the election rule changes.